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THE

AMERICAN JOURNAL OF SCIENCE.

[THIRD SERIES.]

ART. XLVI.—*On the Destruction of the Passivity of Iron in Nitric Acid by Magnetization*;* by EDWARD L. NICHOLS and W. S. FRANKLIN.

I.

DURING the winter of 1884–85, the authors of this paper were engaged in the investigation of the chemical behavior of iron in the magnetic field. In the course of their experiments a beaker containing powdered iron, submerged in cold concentrated nitric acid, was placed between the poles of a small electro-magnet. While in the magnetic field the iron was touched with the bulb of a thermometer, used in the determination of the temperature of the acid, whereupon it lost its passivity and the solution burst into violent effervesence. The reaction continued until the beaker was removed from the field, when the iron again became passive. This change of condition was found to be due to the action of the magnet, occurring instantly in solutions of high temperature or of small degrees of concentration, and taking place upon stirring in the field, even in the case of iron in cold, concentrated acid. The further study of this phenomenon, of which brief mention has

* Paper read before the Kansas Academy of Science, November, 1885, with a supplementary note of later date.

been made in the published account of the above mentioned investigation,* forms the subject of the present paper.

The influence of the electric current upon the passivity of iron has been known for half a century, Schoenbein having described the phenomenon at some length in the well-known letter to Faraday, in which the discovery of the passive state was first announced.† Faraday followed up Schoenbein's work with a variety of ingenious experiments of his own, and reached the conclusion that the passive state is due to the formation of a film of insoluble oxide upon the metal, and that the power of the electric current to produce passivity in iron, which acts as a positive electrode, lies in the liberation of free oxygen upon its surface. This view of the matter has been confirmed by the researches of Boutmy and Chateau,‡ who pointed out the power of chromic, sulphuric and permanganic acids to promote passivity, and by the more recent investigations of Renard,§ in which it was shown that all reducing agents have a tendency to destroy the passivity of iron, whereas oxidizing agents tend to bring about the passive state.

II.

The behavior of iron in nitric acid solutions varies in the most striking manner with the temperature and strength of the acid and with the molecular condition of the metal.

The active metal may be dissolved with evolution of nitrous fumes and the production of a ferric salt, with the evolution of hydrogen and the production of a ferrous salt, or without the evolution of any gas.‖

In anticipation of similar variations in the transition from the passive to the active state, it was thought best, before entering upon the study of the influence of magnetization upon passivity, to make the following preliminary experiments:

(1). *On the behavior of passive iron in cold nitric acid.*—One gram of powdered iron was placed in 8^{cc} of nitric acid, sp. gr. 1·368, and allowed to stand. The metal was apparently entirely passive and remained so throughout the experiment. The temperature of the solution was 20° C. Upon standing, the acid began to show signs that the passivity was not complete; at the end of an hour the amount of iron in solution being sufficient to color the acid unmistakably. Repetitions of the experiment, always with the same result, led to the conclusion that in

* On the Chemical Behavior of Iron in the Magnetic Field, this Journal, April, 1886, vol. xxxi, p. 272.

† Faraday: Experimental Researches, vol. ii, pp. 237–244.

‡ Boutmy and Chateau: Cosmos, xix, p. 117.

§ Renard: Comptes Rendus, vol. lxxix, No. 5.

‖ Ordway: this Journal, II, vol. xl, p. 316.

the case of finely divided "iron by alcohol," in strong acid, Ordway's third reaction (the solution of the iron without the evolution of gas) was always going on.

(2). *On the destruction of passivity by heat.*—A test tube containing passive iron in strong nitric acid was placed in a water-bath and heated. Rise of temperature was found to increase the rapidity of the reaction without evolution of gas. This was gradually supplanted by the second reaction described by Ordway (with the elimination of hydrogen), effervesence becoming very evident at 60°. Under these conditions the metal could not be regarded as "passive" in the strict sense of the word, and yet the reaction differed entirely from that of strong acid upon active iron. At this temperature the concentrated acid acted upon the passive iron in a way similar to that in which a very dilute acid attacks the active metal.

The transition between these two reactions was not sharply marked. Further heating increased the effervesence until, within the neighborhood of 80°, nitrous fumes began to be evolved. The reaction was still far from equaling that which such acid is capable of exerting upon active iron, but the condition of the solution became more and more unstable, and a slight further rise of temperature sufficed to render the iron subject to the full effect of the acid. This final and complete loss of passivity was very sharply marked by the setting in of a reaction of fairly explosive violence.

Repetitions of this experiment showed that the temperature, at which these successive changes occur, depends upon a variety of circumstances. It was found to vary with the time the iron had been exposed to the acid, with the strength of the latter, and with the character of the iron. Even the presence of small quantities of the salts produced by these reactions was found to influence in a marked degree the temperature at which, in a subsequent trial, the metal became active.

(3). *On the influence of time.*—The influence of the time of exposure upon the temperature of the loss of transition was first noticed during the experiment just described. A test tube containing passive iron in nitric acid had been allowed to stand for some time before being placed in the water-bath. Instead of retaining its passivity to a temperature of nearly 90°, this specimen became active at a much lower temperature. Repeated trials showed that continued exposure always lowered the temperature at which passivity was lost. To determine the extent of this variation, other conditions were kept as nearly uniform as possible and the iron was exposed, for different intervals of time, to the acid before the destruction of its passivity. The control of conditions but imperfectly understood was far from complete, and the results of the determination

showed irregularities which would doubtless disappear—had the time been the only remaining source of variation. The series given in the following table is thought, however, to indicate, with a fair degree of accuracy, the influence of the time of exposure upon the temperature of loss of passivity. The time given is that which elapsed between the placing of the iron in the acid and its change to the active condition. It will be seen that the temperature of transition falls as the time of exposure increases.

TABLE I.

Influence of the duration of exposure to the acid upon the temperature at which iron becomes active in NHO_3, sp. gr. 1·368.

DURATION OF EXPOSURE.	TEMPERATURE OF TRANSITION.
$0^m\ 45^s$	89·4°
$1^m\ 2^s$	84·2°
$4^m\ 25^s$	79·3°
$6^m\ 25^s$	78·7°
$9^m\ 30^s$	78·3°
$13^m\ 50^s$	71·5°
$25^m\ 30^s$	70·8°

(4). *On the influence of the presence of iron salts in the solution.*—The presence of even a small amount of the final products of the action of nitric acid upon iron was found to lower the temperature of transition to the active state to a marked extent. In order to obtain consistent results it was necessary to clean the test-tubes in which the reaction was to take place, and the thermometer, by boiling in sulphuric acid. Failure to thoroughly cleanse the latter repeatedly reduced the temperature of transition many degrees, in one instance to 60°, and in a test-tube from which the products of a former reaction had not been entirely removed, it was often impossible to render iron passive even at 20°.

(5). *On the influence of the strength of acid.*—A more obvious cause of variation in the temperature of transition than those already described, lies in the different strengths of acid in which the iron is submerged. A set of experiments upon this variation gave the following results:

TABLE II.

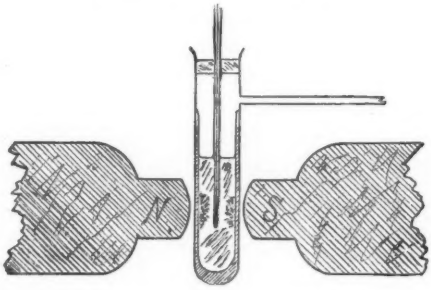
Temperature of transition from the passive to the active state in acid of different degrees of concentration.

SP. GR. OF ACID.	TEMPERATURE OF TRANSITION.
1·368	89·5°
1·354	82·3°
1·306	69·6°
1·130	23·0°

The time of exposure to the acid was in each case about 40 seconds. This determination agrees well with that noted by Schoenbein for iron wire in nitric acid, sp. gr. 1·35. He found the change to occur at 75°.

III.

In these preliminary experiments the method pursued had been very simple. The powdered iron was placed in a perfectly clean and dry test-tube, and the acid having been measured out was poured upon it. The test-tube was then placed in a water-bath, a thermometer was inserted in the acid, and the transition-temperature, always indicated by an explosion of such violence as to expel the greater portion of the contents of the tube, was noted. In order to protect the observer from the explosion, an inverted glass funnel was set over the whole, part of the thermometer projecting beyond the tube of the funnel. In carrying on similar experiments in the field of an electro-magnet, more complete protection from the acid fumes was obtained by means of the apparatus shown in the accompanying figure. The test-tube holding the iron and acid was



placed in a larger tube between the pole-pieces of the magnet. Into the mouth of the larger tube there was fitted a thermometer by means of a rubber cork. A long and narrow glass tube, sealed into the outer tube just below the cork, afforded an exit to the gaseous products of the reaction, which were caught over a pneumatic trough.

The electro-magnet was designed with special reference to this work. The diameter of the cores was 55^{mm}, and the pole pieces were so arranged that the tube containing the iron and acid could be brought in a nearly uniform field of high intensity. The magnet was actuated by a battery of "bichromate" cells, and the current-strength regulated by a rheostat of simple construction. The intensity of the magnetic field was esti-

mated by withdrawing a coil of small area from the field and comparing the consequent swing of a galvanometer needle with that produced in the same needle by a Weber's "earth-inductor."

With this apparatus the influence of magnetization upon the passivity of iron was studied under various circumstances. It was found very difficult, even with the knowledge of other causes of variation in the temperature of transition, gained by the preliminary experiments, to obtain entirely concordant results; but a number of interesting facts concerning the influence of the magnet were established. The following is a résumé of notes made during the investigation:

(*a*). 8^{cc} of HNO^3, sp. gr. 1·368, acting upon 1 gram of iron in the magnetic field. The tube containing the iron and acid was immersed in water, which up to the beginning of the experiment, had been boiling. The strength of the field, which was insufficient to destroy passivity at the temperature of the room (20°), was not measured.

Effervesence began at once and increased until the thermometer indicated 51°, when the explosion, characteristic of loss of passivity occurred, blowing most of the liquid from the tube. The remainder became quiescent again, but when touched with the thermometer-bulb its explosive activity returned. The reaction differed apparently in its nature from that taking place out of the field. The products of the reaction when poured from the tube, left the tube and thermometer-bulb clean, whereas, after the ordinary reaction they were thickly coated with a black residue, the removal of which necessitated long boiling in acid.

(*b*). A repetition of experiment (*a*), with the magnet not acting. The thermometer rose steadily to 89°, at which temperature the transition to the active state took place.

(*c*). Conditions those of experiment (*a*), magnet acting. Temperature of transition was 51°. It was noticed that passivity was not lost throughout the mass of powdered iron at once, but began at a single point on the side of the tube, whence streams of gas were evolved while the rest of the iron remained passive. From this, as a center, the action spread, until the entire mass became involved. The temperature noted was that at which activity had become general.

(*d*). Conditions those of the experiment (*a*), excepting that the magnet was inactive until the temperature of the solution reached 60°, when the circuit was closed. Passivity was destroyed at once by the action of the magnet.

(*e*). Conditions those of experiment (*a*), excepting that the circuit was made and broken at every degree from 40° upward. But little effect was produced until in the neighborhood of 60°, when each tap of the key was marked by mo-

mentary effervesence and by a sudden rise of the thermometer, which effects became more and more marked as the solution became hotter. Complete and permanent activity, such, namely, as was capable of maintaining itself without the continued action of the magnet, did not occur until a temperature of 87·5° had been reached.

(*f*). A repetition of experiment (*a*), but with a weaker field. The current-strength was 0·28 of that used in experiment (*a*). The magnet was active throughout the experiment. The temperature of transition to the active state was 84·5°.

(*g*). The acid and iron were placed in a bath at 20° and placed between the poles of the magnet. The current was much stronger than that employed in the experiment (*a*). The intensity of the field, determined by the method already described, was 20,000H. Loss of passivity in this field occurred instantly upon closing the circuit.

(*h*). The acid was diluted with one-half its bulk of water, and the iron, having been introduced under the protection of a strip of platinum, remained passive when the latter had been withdrawn. The temperature of the acid was 23°. The iron lost its passivity instantly in a field of small intensity, and regained it only when removed from the neighborhood of the magnet. The very slight residual magnetism of the electro-magnet was found to be sufficient to destroy passivity in this weak acid, the action recurring violently whenever the tube was brought between the poles of the magnet, even when the circuit was open.

Upon repeating this experiment with somewhat stronger acid the strength of field requisite to the destruction of passivity was found to be much greater. In three parts of acid and one of water the iron remained passive under the residual magnetism of the magnet, and even in the field produced by the current last employed. The tangent galvanometer, when transmission took place, indicated 0·16 of the current which had been necessary, in experiment (*a*), to destroy passivity in concentrated acid at 51°, and the field was approximately of half the strength of that under which in the strong acid (experiment *f*), iron had remained passive to 84·5°. In four parts of acid and one of water at a temperature of 23·3°, the iron remained passive until the current-strength reached ·23 of that used in experiment (*a*).

(*i*). To determine in what strength of acid iron would become active at ordinary temperatures in the earth's field, the usual quantity of iron was placed in 10^{cc} of concentrated acid, and the acid rapidly diluted with water, and with such small amounts of snow as were necessary to keep the mixture at 23°. Dilution was continued until the iron had become active.

The transition was much less clearly marked than at high temperatures or within the magnetic field, but repeated trials gave 13^{cc} as the amount of water to be added to destroy passivity.

(*j*). To 20^{cc} of concentrated nitric acid at 0°, containing passive iron, water was added, with snow in quantities sufficient to prevent any marked rise of temperature. The addition of 172·3^{cc} of water and snow did not produce noticeable activity. When placed in the field of the electro-magnet (strength of field approximating 20,000H), effervescence began at once, and a dark green solution of higher density than the acid was formed with a brown magnetic precipitation. By a repetition of the experiment outside of the field, it was found that a trace of color could be detected when the acid was diluted to $\frac{1}{3}$, and that effervesence began when the strength of acid had been reduced to $\frac{1}{13}$. Further reduction to $\frac{1}{20}$ produced no other changes in the reaction, nor did it at any time approach in rapidity that taking place in like circumstances within the field.

It appears from these experiments that the action of the magnet is to lower the temperature of transition to the active state, and that the intensity of the magnetic field necessary to convert passive into active iron at a given temperature increases rapidly with the concentration of the acid. The establishment of a satisfactory theory of the influence of magnetization upon passivity will probably demand a more complete acquaintance with the phenomena touched upon in this paper, and a wider knowledge than we now possess of the subject of chemical action in the magnetic field.

University of Kansas, November, 1885.

Supplementary Note.

An investigation of the electric currents set up between iron electrodes within the magnetic field, completed by us since the presentation of this paper, seems to offer a very satisfactory explanation of the manner in which the chemical behavior of iron is modified, and its passivity destroyed in the magnetic field. We find that when two iron bars placed parallel to the lines of force in the field are submerged in any liquid capable of attacking iron, and when the ends only of one of the bars and the middle of the other bar are exposed to the liquid, the bar with ends exposed becomes in its voltaic relation to the other bar, as *zinc to platinum;* so that if the bars be connected by means of wires through a galvanometer, a permanent current will be found to flow in the circuit thus formed.

In the case of a single mass of iron exposed to an acid within the magnetic field, local currents will be set up between those parts of the metal in which magnetic poles are induced and the

intermediate neutral parts of iron, the poles becoming as *zinc* to the other portions, and the currents flowing from the poles through the liquid to the neutral parts of the metal. There is good reason to think that these local currents are the cause of the curious modifications in the chemical behavior of iron in the magnetic field, already described in the pages of this journal, and of the influence of magnetization upon the passivity of that metal. A full account of this research will be published at an early day.

June, 1887.

ART. XLVII. — *On a Method of making the Wave-length of Sodium Light the actual and practical standard of length;* by ALBERT A. MICHELSON and EDWARD W. MORLEY.

THE first actual attempt to make the wave-length of sodium light a standard of length was made by Peirce.* This method involves two distinct measurements: first, that of the angular displacement of the image of a slit by a diffraction grating, and second, that of the distance between the lines of the grating. Both of these are subject to errors due to changes of temperature and to instrumental errors. The results of this work have not as yet been published; but it is not probable that the degree of accuracy attained is much greater than one part in fifty or a hundred thousand. More recently, Mr. Bell of the Johns Hopkins University, using Rowland's gratings, has made a determination of the length of the wave of sodium light which is claimed to be accurate to one two hundred thousandth part.† If this claim is justified, it is probably very near the limit of accuracy of which the method admits. A short time before this, another method was proposed by Macé de Lepinay.‡ This consists in the calculation of the number of wave-lengths between two surfaces of a cube of quartz. Besides the spectroscopic observations of Talbot's fringes, the method involves the measurement of the index of refraction and of the density of quartz, and it is not surprising that the degree of accuracy attained was only one in fifty thousand.

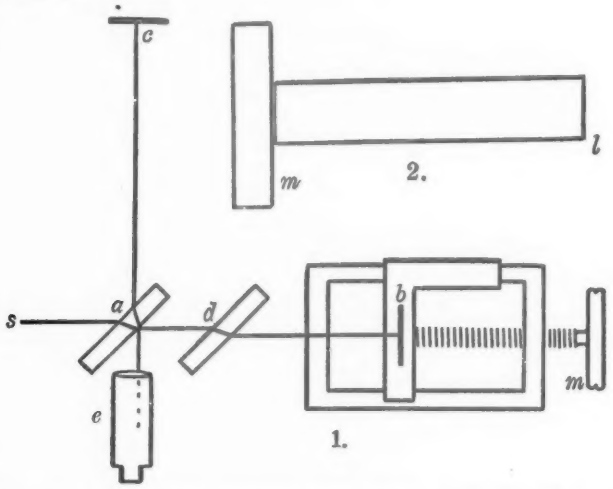
Several years ago, a method suggested itself which seemed likely to furnish results much more accurate than either of the foregoing, and some preliminary experiments made in June have confirmed the anticipation. The apparatus for observing the interference phenomena is the same as that used in the experiments on the relative motion of the earth and the luminiferous ether.

* Nature, xx, 99, 1879; this Journal, III, xviii, 51, 1879.

† On the absolute wave-lengths of light, this Journal, III, xxxiii, 167, 1887.

‡ Comptes Rendus, cii, 1153, 1886; Journ. de Phys., II, v, 411, 1886.

Light from the source at *s* (fig. 1), a sodium flame, falls on the plane parallel glass *a*, and is divided, part going to the plane mirror *c*, and part to the plane mirror *b*. These two pencils are returned along *cae* and *bae*, and the interference of the two is observed in the telescope at *e*. If the distances *ac* and *ab* are made equal, the plane *c* made parallel with that of the image of *b*, and the compensating glass *d* interposed, the interference is at once seen. If the adjustment be exact, the whole field will be dark, since one pencil experiences external reflection, and the other internal.



If now *b* be moved parallel with itself a measured distance by means of the micrometer screw, the number of alternations of light and darkness is exactly twice the number of wave-lengths in the measured distance; thus the determination consists absolutely of a measurement of a length and the counting of a number.

The degree of accuracy depends on the number of wave-lengths which it is possible to count. Fizeau was unable to observe interference when the difference of path amounted to 50,000 wave-lengths. It seemed probable that with a smaller density of sodium vapor this number might be increased, and the experiment was tried with metallic sodium in an exhausted tube provided with aluminum electrodes. It was found possible to increase this number to more than 200,000. Now it is very easy to estimate tenths or even twentieths of a wave-length, which implies that it is possible to find the number of wave-lengths in a given fixed distance between two planes with

an error less than one part in two millions and probably one in ten millions. But the distance corresponding to 400,000 wave-lengths is roughly a decimeter, and this cannot be determined or reproduced more accurately than say to one part in 500,000. So it would be necessary to increase this distance. This can be done by using the same instrument together with a comparer.

The intermediate standard decimeter lm (fig. 2) is put in place of the mirror b. It consists of a prism of glass one decimeter long with one end l plane, and the other slightly convex, so that when it touches the plane m, Newton's rings appear, and these serve to control any change in the distance lm, which has been previously determined in wave-lengths.

The end l is now adjusted so that colored fringes appear in white light. These can be measured to within one-twentieth of a wave-length, and probably to within one-fiftieth. The piece lm is then moved forward till the fringes again appear at m; then the refractometer is moved in the same direction till the fringes appear again at l, and so on till the whole meter has been stepped off. Supposing that in this operation, the error in the setting of the fringes is always in the same direction, the whole error in stepping off the meter would be one part in two millions. By repetition this could of course be reduced. A microscope rigidly attached to the carriage holding the piece lm would serve to compare, and a diamond attached to the same piece would be used to produce copies. All measurements would be made with the apparatus surrounded by melting ice, so that no temperature corrections would be required.

Probably there would be considerable difficulty in actually counting 400,000 wave-lengths, but this can be avoided by first counting the wave-lengths and fractions in a length of one millimeter and using this to step off a centimeter. This will give the nearest whole number of wave-lengths, and the fractions may be observed directly. The centimeter is then used in the same way to step off a decimeter, which again determines the nearest whole number, the fraction being observed directly as before.

The fractions are determined as follows: the fringes observed in the refractometer under the conditions above mentioned can readily be shown to be concentric circles. The center has the minimum intensity when the difference in the distances ab ac is an exact number of wave-lengths. The diameters of the consecutive circles vary as the square roots of the corresponding number of waves. Therefore, if x is the fraction of a wave-length to be determined, and y the diameter of the first dark ring, d being the diameter of the ring corresponding to one wave-length, then $x = \frac{y^2}{d^2}$.

There is a slight difficulty to be noted in consequence of the fact that there are two series of waves in sodium light. The result of this superposition of these is that as the difference of path increases, the interference becomes less distinct and finally disappears, reappears, and has a maximum of distinctness again, when the difference of path is an exact multiple of both wave-lengths. Thus there is an alternation of distinct interference fringes with uniform illumination. If the length to be measured, the centimeter for instance, is such that the interference does not fall exactly at the maximum—to one side by, say, one-tenth the distance between two maxima, there would be an error of one-twentieth of a wave-length requiring an arithmetical correction.

Among other substances tried in the preliminary experiments, were thallium, lithium, and hydrogen. All of these gave interference up to fifty to one hundred thousand wave-lengths, and could therefore all be used as checks on the determination with sodium. It may be noted, that in case of the red hydrogen line, the interference phenomena disappeared at about 15,000 wave-lengths, and again at about 45,000 wave-lengths: so that the red hydrogen line must be a double line with the components about one-sixtieth as distant as the sodium lines.

ART. XLVIII.—*The work of the International Congress of Geologists;* by G. K. GILBERT.

[Address delivered before the Section of Geology and Geography of the American Association for the Advancement of Science, at the New York meeting, August 10, 1887.]

ELEVEN years ago this Association met at Buffalo. It was the year of the Centennial Exhibition, and we were honored by the presence of a number of European geologists. This naturally opened the subject of the international relations of geology, and the proposition to institute a congress of geologists of the world took form in the appointment by the association of an International Committee. The project thus initiated found favor elsewhere, and there resulted an international organization, which up to the present time has held three meetings. It convened first at Paris in 1878, then at Bologna in 1881, and at Berlin in 1885.* Its next meeting will be held in London next year, and an endeavor will be made to secure for the United

* The report of the Paris meeting, entitled *Congrès international de Géologie* is a document of the *Exposition universelle internationale de* 1878, *à Paris*, and covers 313 pages, 8°. The report of the Bologna meeting was printed by the Congress: "*Congrès géologique international, compte rendu de la 2me session, Bologna*, 1881;" pp. xv + 663, 8°. The official report of the Berlin meeting has not yet appeared; but its proceedings are abstracted in "The work of the International Congress of Geologists, and of its Committees. Published by the American

States the honor of the fifth meeting. The original committee of the association has been continued, with some change of membership, and has sent representatives to each session of the congress.

The work of the congress, as originally conceived and as subsequently undertaken, has for its scope geologic nomenclature and classification, and the conventions of geologic maps. The particular classifications attempted are the establishment of the major divisions used in historic and stratigraphic geology and the subdivision of volcanic rocks. In nomenclature three things are undertaken: first, the determination of the names of historic and stratigraphic divisions; second, the formulation of rules for nomenclature in paleontology and mineralogy; and third, the establishment and definition of the taxonomic terms of chronology (period, epoch, etc.), and of stratigraphy (system, series, etc.) The map conventions most discussed are colors, but all signs for the graphic indication of geologic data are considered. The congress has also undertaken the preparation of a large map of Europe, to be printed in forty-nine sheets.

The work was for the most part planned at the Paris meeting, and committees were appointed to formulate subjects for action by the congress at subsequent sessions. Briefly stated, the work accomplished to the present time is as follows: Agreement has been reached as to the rank and equivalence of the taxonomic terms employed in chronology and stratigraphy, a set of rules for paleontologic nomenclature has been adopted, and many sheets of the map of Europe have been prepared for the engraver. A partial classification of stratified rocks has been agreed to and also a partial scheme of map colors, but the reports of proceedings indicate that action in these matters is tentative rather than final.

It is understood that both of these subjects will have prominent place in the proceedings at the London meeting, and the American committee is endeavoring to prepare itself for representative action at that meeting by ascertaining the opinions of all American geologists on the various subjects. It has asked this Section to set apart a day for the discussion of some of the more important questions, and it can hardly be doubted that the Section will realize the mutual advantage of thus assigning the time requested.* I am personally so impressed with the im-

Committee, under the direction of Persifor Frazer, *Docteur des sciences naturelles* (*Univ. de. France*), Secretary. 1886;" pp. 109, 8°; and its work is summarized in "*Resultats scientifiques du Congrès géologique international de Berlin et des travaux qui s'y rattachent, par E. Renevier, prof.;*" pp. 22, 8°, in *Bull. Soc. Vaudoise Sci. Nat.*, vol. xxii, No. 94.

* A day was assigned, as requested. The Section listened to a report by the secretary of the committee, Dr. Frazer, and began to discuss it point by point, but adjourned without great progress. A resolution was passed approving the work of the Committee.

portance of the possible work of the congress that I shall devote the present hour also to its consideration.

The first thing the congress did was to select names for a set of categories to express the taxonomic rank of stratigraphic divisions, on the one hand, and of chronologic divisions on the other. In the terminology of zoology and botany the words kingdom, class, order, family, genus, species, etc., however difficult of definition they may severally be, nevertheless are used always in the same order of inclusion. No systematist in those sciences would think of grouping orders together and calling them a family, or of styling a group of families a genus. But in geology there is no such uniformity of usage. With some writers a group is larger than a series, with others it is smaller. With some an age includes several periods, with others a period includes several ages. There are even writers who ignore the distinction between stratigraphy and chronology; and among the classifications submitted to the congress is one in which an age is subdivided into systems. There is a manifest advantage in bringing order out of this chaos, and so great is the utility of uniformity and perspicuity that the decisions of the congress in this regard will unquestionably be followed by future authors. The terms and the order adopted by the congress are as follows: Of stratigraphic divisions that with the highest rank is *group*, then *system*, *series* and *stage*. The corresponding chronologic divisions are *era*, *period*, *epoch* and *age*. This order of rank is strange to most English readers and writers, and so is one of the terms—*stage*—but the strangeness is only a temporary disadvantage and will not seriously retard the adoption of the convention. The fact that we have previously used the words in a different sense, or that their etymology might warrant a different meaning, need not deter us, for we know from frequent experience that the connotations of a word transferred from one use to another quickly disappear from consciousness, leaving it purely denotative. The introduction of the word *stage*, which can hardly be said to have had an English status heretofore, or at least the introduction of some new word for that part of the column, was necessitated by the restriction of the word *formation* to a special meaning,—the designation of mineral masses with reference to their origin.

The same restriction vacated another office that had been filled by formation, and to this office no appointment was made. I refer to the use of the word to denote indefinitely an aggregate of strata—as in saying, This formation should be called a series rather than a system. This is an important function, for which some provision must be made. I suggest that we may advantageously enrich our language by the permanent adop-

tion of *terrane*, a word whose English meaning has not been well established.*

The fixation of the chronologic terms creates a similar difficulty. We have crystallized out of our magma the terms *era*, *period*, *epoch* and *age*, and there remain in the ground-mass only *eon*, *cycle* and *time*. Of these, *eon* has a poetic connotation which seems to unfit it for this particular use; *cycle* implies repetition or recurrence; and *time* has been so generally applied to unlimited duration that it is difficult to apply it also to limited duration, even though the nature of the limitation be indefinite. On the whole, *time* seems open to the least objection, but I cannot help regretting that either *period* or *age*, both of which have heretofore passed current in the indefinite sense, was not reserved by the congress for that function. With English-speaking peoples the word *eon* could have been better spared for the definite series.

But while the terms selected by the congress are not beyond criticism, the benefits to be derived from an agreement in an orderly system are so great that I for one shall unhesitatingly adopt them as they stand,—provided, of course, that the congress makes no effort to improve its selection.† A small reform of this nature yields its profit to this as well as future generations, and I hold it a duty to favor even those reforms which involve so much effort and pains that their blessings cannot be realized by those who initiate them. Such are the exchange of our English spelling for a rational system, and the exchange of decimal notation in arithmetic for a binary notation. My application of the new nomenclature begins with this address, in the preparation of which I have experienced its utility. That you may have no difficulty in interpreting my reformed language, I have placed the taxonomic legend on the wall, with the addition of the complementary indefinite terms, terrane and time.

Terranes.	Times.
Group.	Era.
System.	Period.
Series.	Epoch.
Stage.	Age.

There are propositions before the congress to distinguish the names of individual groups, systems, series and stages by means

* In a review of this paper, by Dr. Persifor Frazer, it is pointed out that this suggestion is not original with me, having been made by Prof. Renevier, at the Berlin meeting of the Congress, and that *terrane* is used in this sense in the pamphlet published by the American committee. The review, which consists chiefly of unfavorable criticism, may be found in the *American Naturalist* for September—vol. xxi, pp. 841–847.

† Several national committees favor the interchange of the words *group* and *series*.

of terminations, those of the same rank having the same termination. Thus it is proposed by a committee that every name of a group shall end in *ary*.—Tertiary, Primary, Archeary; it is proposed that names of systems end in *ic*,—Cretacic, Carbonic, Siluric; it is proposed that names of series end in *ian*,—Eifelian, Laramian, Trentonian; and it is proposed that stage names terminate with *in*. Another committee suggests that *ic* be used for stages instead of systems. The adoption of such a plan would enable a writer or speaker to indicate the taxonomic rank of a terrane without adding a word for that purpose. If he regarded a certain terrane taking its name from Cambria as a system, he would call it the Cambric; if he esteemed it only a series, he would say Cambrian; and there would be no need of adding the word system, or series, in order to express his full meaning. Conversely the reader or hearer would always learn its taxonomic rank, or supposed rank, whenever a terrane was mentioned. These I conceive to be the advantages derivable from the change, but they would not be the only effects. It would become impossible for a geologist to name or allude to a terrane without declaring its rank, and the consequences of this would be evil in many ways. In the first place one could not discuss terranes from any point of view without expressing an opinion as to their taxonomy, and the change would thus contravene one of the most important rights of opinion—namely, the right to reserve opinion. Again, geologists who differed as to the rank of a terrane would necessarily terminate its title differently, and a needless synonymy would thus be introduced. In the third place the created necessity for taxonomic discrimination on all occasions would tend to direct undue attention to taxonomic problems. Taxonomy would be conceived by many geologists as an end instead of a means, just as correlation has been conceived, and energy would be wasted in taxonomic refinement and taxonomic controversy. It is convenient for purposes of description and comparison to classify the strata that constitute a local columnar section in phalanges of various magnitude or rank, but the criteria on which we depend for discrimination are in the nature of things variable and offer ground for endless difference of opinion; and it would be extremely unfortunate to have such differences perpetually brought to the foreground.

Another subject considered by the congress is the nomenclature of paleontology. A committee appointed for the purpose formulated rules for the establishment of the names of genera and species, and their report was adopted by the congress. I have no opinion to express as to the wisdom of the rules, but it is a matter of surprise that a body of geologists assumed to speak with authority on the subject. From one point of view

paleontology is a part of geology; from another point of view it is a part of biology. In so far as it names genera and species it is purely biologic, and it would seem proper that the students of fossils unite with the students of living animals and living plants in the adoption of rules of nomenclature.

A similar remark applies to the nomenclature of mineralogy, in regard to which no action has yet been taken. The most intimate relations of systematic mineralogy are with chemistry.

Yet another projected work of the congress is the classification of eruptive rocks. Up to the present time action has been deferred, and it may reasonably be hoped that no scheme of classification will be adopted. If there existed a system of classification which gave general satisfaction and had stood the test of time, there would be little harm—and little or no advantage—in giving it the official stamp of approval. If the main features of a classification were well established and the residuary discrepancies were recognized as unessential, it is conceivable that some benefit might be derived from the submission of the matter to an assembly of specialists. But the actual case is far different. Not only is there wide difference as to the classification of volcanic rocks, but there is no agreement as to the fundamental principles on which their classification should be based, for we still lack an accepted theory of volcanism. At the same time observation is being pushed with great vigor, and with the aid of new and important methods. With the rapid growth of knowledge and ideas classifications are continually remodelled, and the best is in danger of becoming obsolete before it has been printed and circulated. Should the congress enter the lists, one of two things would occur. Either its classification would be treated like that of an individual and ignored as soon as a better one was proposed, or it would be regarded as more authoritative, and new facts would for a time be warped into adjustment with it. In either case the reputation of the congress would eventually suffer, and in one case science would suffer also.

There remain to consider the two most important undertakings of the congress, the classification of terranes and the uni fication of map colors. The congress is attacking these subjects indirectly by means of a third undertaking, the preparation of a geologic map of Europe, and this method of approach has had the effect of making it difficult properly to interpret its action. There can be no doubt that those who originally organized the work contemplated the enactment of a stratigraphic classification to be applied to the entire earth and the selection of a color scheme for use either in all geologic maps or in all general geologic maps. But at the Berlin session the committee in charge

of work on the map of Europe pressed the congress for the determination of questions on which hung the completion of the map, and many hasty decisions were reached, while not a few disputed points were referred to the map committee. The debates indicate that much or all of this work was provisional or of merely local application, but the resolutions adopted show little qualification. It should be added that the official minutes of the meeting are still unpublished. In view of the uncertainty thus occasioned I shall not attempt to characterize the attitude of the congress on the subject of classification, but shall merely develop my individual view.

It is the opinion of many who have discussed the general classification of terranes by convention of geologists that the smallest unit of such classification should be the stratigraphic system. What is a stratigraphic system? The congress implies a definition in saying that a system includes more than a series and less than a group, and that the Jurassic is a system; but this gives only a meagre conception and we need a full one. As the problem of classification demands a true conception of a system, and as there is reason to believe that a false conception is abroad, it is proper that in seeking the true one we begin with the elements.

The surface of the land is constantly degraded by erosion, and the material removed is spread on the floor of the ocean, forming a deposit. This process has gone on from the dawn of geologic history, but the positions and boundaries of land and ocean have not remained the same. Crust movements have caused the submergence of land, and the emergence of ocean bottom, and these movements have been local and irregular, districts here and there going up while other districts went down. The emergence of ocean bottom exposes the deposit previously made on it and subjects it to erosion. In this way every part of the known surface of the globe has been the scene of successive deposition and erosion, and in many districts the alternations of process have been numerous. It is manifestly impossible that either erosion or deposition should have ever prevailed universally, and it has been established by the study of stratigraphic breaks that a time of erosion has often interrupted deposition in one region while deposition was uninterrupted in another.

In transportation from its region of erosion to its place of deposition detritus is assorted, and it results that the simultaneous deposits on the bottom of an ocean are not everywhere the same. Equal diversity is shown in the ancient deposits constituting geologic formations. It is a general fact that synchronous formations have not everywhere the same constitution.

Many of the variations in deposits are correlated with depth

of water and distance from shore, and it results that elevation and subsidence in regions of continuous deposition produce changes in the nature of the local deposit.

The animals and plants of the earth are not universally distributed, but are grouped in provinces. In the geologic past similar provinces existed, but their boundaries were different, shifting in harmony with the varying geography of the surface. From time to time the barriers separating contiguous provinces have been abolished, suffering them to coalesce; and conversely new barriers have arisen, creating new provinces. From the earliest paleozoic to the present time the species of animals and plants have been progressively modified, the nature of the modification depending on local conditions. The faunas and floras of different provinces thus become different, and the longer the provinces remain distinct, the greater is the divergence of life. The removal of a barrier either produces a new fauna by the fusion of the two previously separated, or else obliterates one and extends the area of the other. In either case there is a change toward the unification of life, and in either case there is an abrupt change in a local fauna. Thus the secular evolution of species, combined with the secular and kaleidoscopic revolution of land areas, leads to two antagonistic tendencies, one toward diversity of life on different parts of the globe, the other toward its uniformity. The tendency toward uniformity affords the basis for the correlation of terranes by comparison of fossils; the tendency toward diversity limits the possibilities of correlation.

If now we direct attention to some limited area and study its geology, we find that under the operation of these general processes it has acquired a stratigraphic constitution of a complex nature. Its successive terranes are varied in texture. Breaks in the continuity of deposition are marked by unconformities. The fossils at different horizons are different, and when they are examined in order from the lowest to the highest, the rate of change is found to vary, being in places nearly imperceptible and elsewhere abrupt. It is by means of such features as these—that is, by lithologic changes, by unconformities, and by life changes—that the stratigraphic column is classified into groups, systems, series and stages. A system is a great terrane separated from terranes above and below by great unconformities, or great life breaks, or both. Smaller unconformities, smaller life changes, and lithologic changes are used for the demarcation of series and stages; and on the other hand, exceptionally great unconformities and life breaks are used to delimit groups. As the same criteria determine groups, systems and series, differing only in degree, the precise definition of the term system is impossible, and in many cases the gradation of a terrane as a group, a system or a series is largely a matter of convenience.

From this point of view a system is somewhat artificial, but there is a more important sense in which it is natural. It is limited by stratigraphic or paleontologic breaks above and below, and these breaks are natural. The taxonomist is not warranted in dividing systems where no such break exists.

Transferring now our attention to some other area, distant from the first, and studying its stratigraphy, we find that the same principles enable us to divide it independently into stages, series, systems and groups. Its fossils are not the same, but they are to a certain extent similar, and the sequence of life is approximately parallel. We cannot compare stage with stage, nor series with series perhaps, but we can compare system with system, and making the comparison we discover that the breaks are at different places. While one area was upraised and subjected for a time to erosion, the other received continuous deposition. While life in one area, enjoying constant conditions, was almost unchanged for long ages and even epochs, it was revolutionized in the other by the irruption across some obsolescent barrier of strong and aggressive faunas and floras. The systems of one area, therefore, do not coincide with the systems of the other in their beginning and ending. They may differ in number, and they may differ greatly in magnitude and in the duration they represent. They are in fact a different set of systems.

The case I have described is ideal but not false. It represents the common experience of those who have developed the geologic histories of remote districts and attempted to correlate them with the geologic history of Europe. There does not exist a world-wide system nor a world-wide group, but every system and every group is local. The classification developed in one place is perfectly applicable only there. At a short distance away some of its beds disappear and others are introduced; farther on its stages cannot be recognized; then its series fail and finally its systems and its groups.

If I have properly characterized stratigraphic systems—if they are both natural and local—it goes without saying that the classification of the strata of all countries in a dozen or so systems, as proposed by some of the members of the congress, is impossible.

I hasten to add that from the point of view of these gentlemen what they advocate is not necessarily impossible, for they have a different conception of a system. They regard it not as local but as universal. It is their privilege to define their terms as they please, and we will not dispute about mere words, but I cannot too strongly or too earnestly insist that a system which is universal is artificial. It may be natural in one geologic province, but it is artificial in all others. Take for example the

Jurassic. It is a natural system in Europe. In the eastern United States no strata are called Jurassic with confidence, and at the west the rocks called Jurassic merge with those called Triassic. In India, Medlicott tells us, a Jurassic fauna occurs at the summit of a great natural system containing a Permian fauna near its base.* In New Zealand, according to Hutton, a continuous rock system, dissevered by great unconformities from other systems, bears at top fossils resembling those of the lower Jurassic and lower down fossils of Triassic facies.† To establish a Jurassic system in either of these countries it is necessary to divide a natural system, and a Jurassic system thus established would be necessarily artificial.

This is the sort of classification implied by the assumption that systems are world-wide. It is not impossible, but it is highly unadvisable. It is classification for the sake of uniformity, and its uniformity is procrustean. The natural systems of a region are the logical chapters of its geologic history. If you group its strata artificially according to the natural divisions of another region, you mask and falsify its history. The geologic history of the earth has as great local diversity as its human history. As in human history there are interrelations and harmonies and a universal progress, but these are perceptible only in the general view; and the student whose preconceptions lead him to exaggerate the harmonies and ignore the discrepancies perverts the meaning of every page.

I prefer therefore my own definition of system, making it natural and consequently local, and I earnestly oppose any attempt to coerce the geology of one country in a rigid matrix formed over and shaped by the geology of another country.

The ideas I oppose have arisen in connection with the work of correlation. Some geologists appear to regard correlation as the determination in distant localities of identities; the more philosophic regard it as the determination of the actual relations, whether they be of identity or difference. With the former the basis of correlation is the universality of geologic systems; with the latter it may be said to be the universality of geologic time.

Now in the comparative study of local geologic histories, just as in the comparative study of local human histories, it is a matter of convenience to have a common scale of time. It is not essential, but it is highly convenient. In human history we use an astronomic scale of equal parts, designating each unit by a number. In geology no scale of equal parts is available, and we employ the eras and periods, and to some extent

* *Sur le coloriage des Cartes geologiques, par M. H. B. Medlicott;* in *Congrès géologique international, Compte rendu de la 2me session, Bologne*, 1881, pp. 652–656.

† Sketch of the Geology of New Zealand. By Captain F. W. Hutton. In Quart. Jour. Geol. Soc. London, vol. xli, pp. 191–220.

the epochs, of the local geologic history first deciphered—that of Europe. These time divisions bear the same names as the groups, systems and series of strata whose deposition occurred within them.

So far as the science of geology is concerned the selection of Europe as its first field of study was a matter of chance, and the adoption of the European time scale as a general standard may therefore be said to have been accidental. Though the local rock scheme on which it is based is natural, the time scale, considered as universal, is arbitrary. Another locality would have afforded a different scale, but its authority would neither be greater or less. The scale being recognized as arbitrary, and a mere matter of convenience, it is legitimate to modify and fix it by formal convention. The Congress can do good service to geologic technology by putting it in the best possible shape and giving it an official status. In my judgment only a small number of divisions should be admitted, not more than the number of periods of the European scheme. In a general way the durations represented by the coördinate divisions should be as nearly equal as practicable, but a certain concession might be made to chronologic perspective on account of our superior opportunities for studying the later history. Some of the shorter periods might perhaps be united under new names. Each line of division between periods should be defined by means of a stratigraphic plane of division, and this can be done with precision if a locality is made part of the definition.

Especially should pains be taken to declare the arbitrary nature of the scale. Even with this precaution it will be misconstrued by many, for there is a tendency of the mind to attach undue weight to classification. Wherever we draw lines of separation we lose to a certain extent the power to recognize continuity. When, for example, the clock strikes twelve on New Year's Eve time seems to stop and begin again. We speak of the achievements of the nineteenth century—and despite ourselves we think of them, too—as though a new industrial epoch began in A. D. 1800. And so it is easy for the beginner in geology to accept as discontinuous the eras and periods of which his text-book treats, and it is hard for him afterward to unlearn the lesson.

There is reason to believe that confusion of ideas in regard to geologic classification has been fostered by the employment of the same set of names for the divisions of the time scale and for the local terranes on which they are founded. It might be well to furnish the time scale with names suggesting time—such names as the brothers Rogers applied to the terranes of Pennsylvania—but so radical a change is hardly feasible, especially

as we should thus lose the mnemonic connection of times with corresponding terranes. I propose as a means of accomplishing the end with the least inconvenience, that a set of time words be derived from the terrane names by modifying the final syllables. The time words should all have the same termination, and that should differ from any terminations occurring in the terrane names. I suggest for the ending of time words the syllable *al*. With such a nomenclature Jurassic and Devonian would denote only certain European rock systems, while Jural and Devonal would denote periods of the standard time scale; and we could speak of the Chico-Tejon series as partly Eocenal and partly Cretaceal without seeming to imply the existence in California of the Eocene and Cretaceous systems of Europe.

A few minutes ago I opposed the differentiation of words by terminations because it abrogated the power of indefinite expression; I now favor it for the same reason. It is well to be indefinite as to the taxonomic rank of terranes while their characters are imperfectly known, but it is not well to confuse terranes with times.

It is not to be assumed that a time scale adopted now as the best possible will continue indefinitely to be the best possible; the day will inevitably come when it can be improved. In the fuller light of the future we may recognize as very unequal periods that we now deem equivalent, and the possibilities of defining pre-Cambrial periods are unlimited. Even now there are announced beneath the lowest fossil-bearing terranes of the Lake Superior region two systems of clastic rocks limited above and below by great unconformities, and Irving demands their recognition as a group, distinct from the Archæan. If his voice is heard, the time scale will include an era between the Paleozoal and the Archeal, and this era will supply the needs of the systematist until great additions have been made to our present knowledge of the older rocks.

My only remaining subject is the representation of terranes on maps by means of colors. At present no two organizations and scarcely two individuals use colors in the same way, and it is probably true that every organization and individual publishing many geologic maps has at different times employed the same color for different terranes and different colors for the same terrane. It results that the map user can gain no information from the distribution of colors until he has studied the legend; before he can read a new atlas he must learn a new alphabet. The advantage to be gained by substituting a universal language for this confusion of tongues is manifest and great, and has justified the application of much time and attention by the Congress and its committees. By a series of

resolutions a partial scheme has been selected, one color at a time, and the completion of the plan has been left to the committe on the map of Europe. That committee has prepared a color legend, which is accessible to American geologists in the volume of information published by the American committee. It is understood in a general way that the Congress reserves final action, and the published legend not only belongs specifically to the map of Europe, but is provisional; still, as this map, if generally approved, will unquestionably be declared by the Congress an authoritive pattern for the guidance of map makers, the plan should be freely criticised at its present stage. The selection of uniform colors is a far more delicate and important matter than the arrangement of taxonomic terms; for while ill-chosen words may quickly fit themselves to new uses, the adoption of an ill-arranged color scheme must entail continual loss.

In my judgment the scheme provisionally chosen is defective in several particulars, to which I shall presently call attention, but it is neccessary to introduce the discussion by a statement of the conditions to be satisfied by a standard color scheme and a statement of the practical means available. The following are the principal conditions, arranged in an order embodying my estimate of their relative importance:

(1). The map must be clearly and easily legible. Each color must be so distinct from each other color that it can be identified, whatever its surroundings; and all other conventions must be readily discriminated.

(2). The cartographic scheme must be adjustable to the geologic facts; it must not require that the facts be adjusted to it.

(3). The same scheme should serve both for general maps, as, for example, those representing only systems, and for detail maps, representing numerous smaller divisions.

(4.) Undue expense should be avoided. The amount and consequent utility of color cartography is largely limited by its cost.

(5). It should be easily fixed and retained in the mind. This is best accomplished by making it orderly.

(6). Other considerations permitting, the map should please the eye. Since the arrangement of color areas cannot be foretold, this can only be accomplished by admitting a certain range of choice. If allowed sufficient latitude in the selection of tones, an expert colorist can ameliorate an offensive combination of hues.

(7). Other considerations permitting, the establishment of a universal system should involve the least possible inconvenience. But as the inconvenience of change is temporary, while the inconvenience of a bad system is lasting, this consideration should yield to every other.

The art of mapping geologic terranes by means of color is well developed, and its methods, viewed from the geologist's standpoint, admit of easy characterization. Color may be varied in two distinct ways—in hue and in tone. Hues differ in quality, as yellowish green and bluish green. Tones differ in strength, as pale green and dark green. A color is printed either solid or broken; it is said to be broken when applied in a pattern, as in lines or dots, or when it is interrupted by a pattern. The difference between solid and broken colors is a difference of texture. The primary discriminations in mapping are through hue, tone and texture.

The map engraver produces texture in three ways. In the first way a single impression is made with a broken color. The white of the paper, displayed where the color is interrupted, combines with the color in the general effect, producing a paler tone of the same hue. In the second way two impressions are made, one with solid color the other with broken, and the two impressions have the same hue; they may or may not differ in tone. This is monochromatic overprinting, and its general effect agrees in hue with the single impression, but differs in tone, being darker. In the third way two impressions are made, one solid, one broken, and their colors differ in hue. This is bichromatic overprinting, and its general effect differs in hue as well as tone from each of the colors combined in it. The first and second ways produce texture monochromatically and do not yield a new hue; the third way produces texture bichromatically and yields a new hue. It is practically impossible to obtain a texture effect without modifying the original tone.

The natural gradation from hue to hue is absolutely continuous and the number of hues is infinite; the number of tones of each hue is likewise infinite. The number of hues and tones the eye can discriminate is finite, but very great; it is stated that one thousand hues have been distinguished in the solar spectrum. But the number of hues and tones that can be combined in a map is small. As a matter of perception, every color is modified by the colors adjacent to it. The same hue affords different sensations when differently surrounded, and different hues may afford the same sensation. The same is true of tones; and there is a certain interdependence of hues and tones in this respect. In a geologic map each color is liable to fall into various combinations, and two colors little differentiated occasion confusion. There is, therefore, a somewhat narrow limit to the employment of hues and tones. The matter has not been fully worked out, but it is probable that twenty is as large a number of hues as can be safely employed in connection with tones. Texture admits of very great variation. The various color schemes submitted to the congress and

printed in the report of the Bologna meeting afford, with their manifest permutations, about two hundred distinct textures, and I am satisfied from a study of these and others that as many as one hundred can be chosen that are not subject to confusion. It follows that a map or atlas expressing few distinctions need use only hues, or only hues and tones, but where numerous distinctions are to be made, recourse must be had to textures.

The printing of a large number of textures of the same hue produces a greater number of tones than can be discriminated, and its effect is to confuse and nullify any distinctions (within the range of that hue) based purely on tone. The printing of a large number of bichromatic textures causes the same result, and it also produces a greater number of hues than can be discriminated; its effect is to confuse and nullify distinctions based purely on tone, or on hue, or on tone and hue together.

In the color scheme prepared for the map of Europe thirty-eight distinctions are made. There are twenty-four hues, and the remaining fourteen distinctions are accomplished by variations of tone. While it may be possible to select twenty-four hues availiable for indiscriminate combination, there can be no question that those provisionally printed by the committee will fail to maintain their distinctness when variously combined upon a map. Under the influence of such chromatic environments as are sure to be encountered, the four yellow hues of the Tertiary cannot be discriminated, and the same difficulty will arise with the two hues of gray assigned to the Carboniferous, and with the hues of gray and brown assigned respectively to the Permian and the Devonian. Some of the tones likewise are not sufficiently distinguished. Two of the blues of the Jurassic, two of the browns of the Devonian, two of the rose tones of the Archæan, and the two violets of the Trias are open to this criticism. A certain amount of adjustment can be made in the final selection of inks, and probably all the defects from tone can be thus remedied, but the confusion of hues is more difficult to eliminate, for the great number of the hues interferes with the separation of those that are too approximate. To strengthen one contrast is to weaken another.

In order to judge of the availability of the scheme for the production of detail maps, it is necessary to consider the resolutions of the congress as well as the printed legend. A resolution provides that the subdivisions of a system shall be represented by shades of the color adopted for the system, or by broken color or other texture devices, and it is further provided that the shades, whether produced by solid color or by texture, shall be so arranged that the darkest or strongest represent the lower divisions of the system.* The resolution is in

* *Congrès géologique international, Compte rendu de la 2me Session, Bologne,* 1881, p. 157.

French, and the word I have translated shade (*nuance*) is one which applies popularly to either hue or tone, while in the scientific terminology of chromatics it applies to hue only. The committee on the map has taken it in its popular sense, and has represented some subdivisions by hues, and others by tones; for example, Pliocene and Miocene are assigned two tones of the same hue, while Oligocene and Eocene have each a separate hue. The upper Cretaceous and part of the lower Cretaceous are assigned a green hue in two tones, while the Gault and the Wealden, classed as subdivisions of the lower Cretaceous, have independent hues of green. Of the six reds assigned to volcanic rocks, two agree in hue and differ in tone, while the remainder have distinct hues. As the legend stands, both major and minor distinctions, that is to say, the discrimination of groups, the discrimination of systems, and the discrimination of divisions smaller than systems, are all accomplished by differences of hue, while the discrimination of minor divisions is accomplished indifferently by variation of hue and by variation of tone. The same means performs several functions, and the same function is performed by several means.

It is stating the same thing from another point of view to say that the Congress and its committees have used the term color in its popular rather than its scientific sense. Scientifically, a color is a particular tone of a particular hue, and the number of colors is infinite. Popularly, a color is an assemblage of contiguous hues and their tones, to which a name has been given. Each hue and tone within the range covered by the name is a shade of the color. It is in this popular sense that the resolutions assign a color to each system, and assign shades of the system-color to the subdivisions of the system.

Now if in the variation of a system-color, by textures or otherwise, a single hue is adhered to, the system-color remains distinct from other system-colors throughout all its modifications and their modifications, but if hues as well as tones are varied, the inevitable result is confusion, for some of the hues of one system-color will approach too near to hues of other system-colors. With a multiplicity of minor distinctions the main distinction of system from system will be lost.

Another difficulty lies in the fact that the Quaternary and Devonian colors, while strongly contrasted in tone, are nearly identical in hue. This does not affect their use in a general map, but in a detail map the stronger tones of the Quaternary gray will approach too closely the paler tones of the Devonian brown.

These criticisms apply to those features of the scheme which affect its adoption for general and detail maps of European countries. There is one of equal or greater importance affect-

ing its application in other continents. It is adjusted to the rock systems of Europe exclusively, and makes no provision whatever for the systems of other parts of the earth. The geologists of Wisconsin, for example, cannot use it without calling the Keweenawan either Cambrian or Archæan. If they were in doubt which division should hold it, but inclined a little one way or the other, they could express their qualified opinion in the notation provided by the map committee; but having attained an unqualified opinion that the terrane belongs to neither of these two categories, they find no means for expressing their conclusions. The scheme cannot be applied to the geology of India, of New Zealand, or of Australia, without misrepresentation. It is not universal but local, and this because it is founded on the fallacy of a world-wide unity of geologic systems.

So far as the geology of the world is concerned, it would be better to adopt no convention at all as regards map colors, than to adopt one carrying with it and promulgating a vicious classification. Uniformity is not worth purchasing at the price of falsification. If the members of the Congress cannot agree upon a plan having the flexibility demanded by the geologic facts, it will be best to limit its action to the local problems involved in the map of Europe. I believe, however, that the necessary flexibility is attainable, and before proceeding to further criticism of the committee scheme I will give the outlines of a plan which appears to me to combine the advantage of flexibility with a number of other desirable qualities.

The plan is founded on the universality of geologic time and the diversity of local geologic histories as expressed in rock systems. Geologic periods are arranged in linear order. Each one adjoins the next and together they constitute continuous geologic time, which we may conceive as represented by a straight line. The stratigraphic systems of a country have likewise an order of succession, and their arrangement is linear. They are not always continuous one with another, but the history recorded by the systems and the breaks between them is continuous and may be represented by a straight line, equal and parallel to that of geologic time. And so for each country. A color scale which shall represent each and all of these parallel lines must be itself linear and continuous, and, fortunately, we have such a scale furnished us in the prismatic spectrum.

I propose, first, that the continuous prismatic spectrum be adopted as the standard universal scale for continuous geologic time. I propose, second, that the conventional time scale, based on the geologic history of Europe, be complemented by a color scale, prismatic but discontinuous. I would assign to

each period, not a certain portion or area of the spectrum, but a specific color defined by its position in the spectrum. This color scale will also apply to the geology of Europe. I propose, third, that the students of each geologic district shall assign to the stratigraphic systems of that district a set of prismatic colors so selected from the spectrum as to properly represent the relation of each system to the time scale, provided that relation is approximately known. Under this rule a system corresponding partly with the Cretaceous and partly with the Jurassic will receive a prismatic color intermediate between those assigned to the Cretaceal and Jural divisions of the time scale. I propose, fourth, that systems whose relations to the standard time scale are not even approximately known be given tentative positions in the time scale and assigned the corresponding colors; and that such provisional colors be distinguished by a special device.

Of this device I will speak later, but before we leave this part of the subject, the capability of the plan to express the facts should be more clearly characterized. Continuous geologic time being equated with the continuous spectral band of light, each period is theoretically equated with a segment of that band including all the hues between certain limits. But, practically, the period is represented in the color scale only by the central hue of the segment, and there is nothing in the nature of this hue to indicate the length of the segment. Similarly each local system is represented only by the hue corresponding to the middle of the equivalent period, considered as a part of the continuous time-scale, and this hue gives no information as to the magnitude of the system or the duration of the corresponding period. When a non-European system is represented on a map with the Devonal color, all that is expressed is that the middle of its period coincides with the middle of the Devonal period; the whole period may equal the Devonal or may be shorter or may be longer. With this limitation the scheme is able to express the exact facts, or the exact state of opinion, in regard to correlation.

I propose, fifth, that the subdivisions of systems be represented, if their number is small, by distinct tones of the hue assigned to the system, and if their number is great, by monochromatic textures. It having been provided that systems shall be distinguished by means of hues, it is now provided that hues shall have no other function. This secures the integrity of the distinction between systems, whatever the minuteness of subdivision.

The idea of using the spectral colors in their proper order is not novel. It has entered into half the plans submitted to the Congress, but each author has introduced other colors also, or

else has undertaken to use the spectrum colors more than once, under the impression that they do not afford the necessary range or variety. This impression is based largely upon the popular meaning of the word color. It is indeed true that if we limit ourselves to those parts of the spectral series which have univocal names, we have only six or seven distinctions; and it is further true that if we have recourse to binomial designations, such as yellowish green and greenish yellow, we obtain rather indefinite conceptions; but to men of science there are better resources than those afforded by the language of every day life. The spectrum has been elaborately studied, and the relations of its dark lines to its colors have been determined. Its wave lengths have, moreover, been measured, and by such means as these we are furnished with three different scales, any one of which is adequate to the precise definition of any hue of the continuous series. What needs to be done is this. When the divisions of the time scale have been decided on, the spectrum must be studied to ascertain the best selection of hues. Their number must, of course, be that of the number of divisions of the time scale, and they must be so chosen that the degree of separateness of adjacent colors shall be everywhere the same, as judged by the normal human eye. Then define each hue by its wave length, or its position in the Kirchhoff scale, and define it also in terms of the best combination of pigments with which it can be approximately reproduced for practical use. It is of course impossible to copy the prismatic colors with accuracy, because the colors of pigments are impure, but this difficulty will not seriously interfere with the employment of the prismatic colors as a standard.

The practical question whether the spectrum will give a sufficient number of hues so far separated from each other as to be distinguishable in all the arrangements occurring on maps has received such consideration as I have been able to give it, and it is my judgment that the maximum number of hues that can safely be used falls somewhere between fifteen and twenty. There will certainly be no difficulty in thus constructing a standard color scale with about a dozen terms.

The employment of the spectral colors in this manner leaves three groups of colors unassigned, the purples, the browns and the grays. If the spectral colors be arranged on the circumference of a circle so that each diameter of the circle connects hues that are complementary, it is found that they occupy the greater part, but not quite all, of the circumference, and the color needed to fill the vacant arc is purple. The hues of purple might then, if deemed necessary, be added to one end or the other of the spectrum, thus increasing the range from which to select colors for the time scale.

My sixth proposition is to assign the browns to volcanic rocks. I would leave the grays unassigned.

It will be observed that no intimation has been given as to whether the violet end of the spectrum should apply to the newest system of strata or the oldest. It must of course be definitely assigned to one or the other, but the particular assignment is a matter of indifference.

The main features of the proposed prismatic scheme have now been set forth and you are fairly entitled to exemption from the minor features, but there is one detail that can hardly be omitted. In one of the main propositions it was provided that some special device should distinguish colors assigned to uncorrelated systems, and I feel it incumbent to show that a suitable device can be found. Of a number that have occurred to me as about equally available, I will mention but a single one—the overprinting, in small dots, widely separated, of the complementary color. The complementary color is selected because it does not disturb the relation of the system-color to the colors of adjacent systems. Bichromatic overprinting produces a hue intermediate between the two hues combined, but the hue midway between a system-color and its complementary color is white or gray, and if only a small amount of the complementary color is added, the system-color becomes merely paler or duller, when viewed from such a distance that the colors blend.

The prismatic color scheme having been constructed for the express purpose of securing a degree of flexibility that will fit it for universal use need not be further compared in that regard with the scheme published by the European map committee. Enough has also been said to show that its superior perspicuity is claimed both for general and for detail maps. A few words will suffice to compare the two systems in other respects.

As regards the expense incurred in the production of general maps, neither scheme has notable advantage, and they are not yet sufficiently developed to permit a comparison as regards the cost of detail maps. Their capability for the production of pleasant color effects can be best judged when maps have been actually made, but it may be said in a general way that the committee's scheme will afford more strong contrasts between adjacent color areas than the prismatic. The maps colored by the former will be relatively lively, those colored by the latter relatively quiet. It is provided by the committee that the volcanic colors shall be not merely red but strong. On a general map volcanic areas cover comparatively small spaces, and strong reds thus disposed will ordinarily add brilliancy; but the detail map of a volcanic district, thus colored, will be disquietingly suggestive of active eruption.

The alphabet of colors for the prismatic scale will be the more easily learned of the two, because it is orderly, and because its order is already familiar in the spectrum. The committee's scheme, however, has some old-fashioned mnemonic features which the prismatic lacks. The green of the Cretaceous is connected with green-sand, the red of volcanic rocks with fire, and the rose of the Archæan with feldspar; and the gray of the Carboniferous mildly suggests the blackness of coal.

In respect to facility of introduction the committee's scheme, being essentially a compromise of existing color scales, has the advantage that to most users it is not entirely novel. The prismatic scheme on the other hand has the advantage of being orderly. It scientifically differentiates the functions of hues and tones, and though each one of its colors may be different from what the individual geologist has previously employed for the indication of the same system, the order of the colors is already familiar to him in another way.

This closes my review of the various works undertaken by the congress. Some of these have been favored, others opposed, and reasons have been given. But there is a general consideration or criterion applicable to all, which has nearly escaped mention, although it is of preëminent importance. When a matter is proposed for regulation by the congress, the first question which should be asked is whether it falls within the legitimate purview of a convention of geologists. It manifestly does not if it belongs to some other science rather than to geology; and objection has on this ground been made against the regulation by our geologic congress of the nomenclatures of paleontology and mineralogy. But not all geologic matters even are properly subject to settlement by convention. This is peculiarly the case with geologic facts. Science is distinguished from the earlier philosophies of mankind by the peculiarity that it establishes its fundamental data by observation. The old philosophies were founded largely upon assumptions, and it was not deemed illogical—perhaps it was not illogical—to appeal to the authority of an assemblage of experts for the establishment of fundamental assumptions. But for science it is not merely illogical, it is suicidal, to establish facts in any other way than by observation. No vote of the most august scientific body can possibly establish a fact, and no vote can have any weight against a good observation.

Now the entire science of geology, using the phrase in a strict sense, is constituted by the aggregation and arrangements of facts, and none of its results can be rendered more true, or be more firmly established, or be prevented from yielding to contradictory facts, by conventional agreement. A class-

ification, if it has any value whatever, is merely a generalized expression of the facts of observation, and is outside the dominion of the voter. If it comprises all the essential facts, its sufficiency will eventually be recognized, whether its authority is individual or collective. If it does not comprise them, it will inevitably be superseded, by whatever authority it may have been instituted. For this reason I am opposed to the classification by the congress of the sedimentary formations, and likewise to the classification of the volcanic rocks, and I also regard it as ill-advised that the congress undertook the preparation of a map of Europe, for that—if more than a work of compilation—is a work of classification.

If we examine the other undertakings of the congress—the definition and gradation of taxonomic terms, the systematization of terminations, the selection of a scale of colors for geologic maps, and the selection of other conventional signs for the graphic expression of geologic phenomena—we find that they all belong to the means of intercommunication of geologists. They affect only the verbal and graphic technical language of the science. Of the same nature is the arbitrary time scale whose preparation I favor,—a conventional terminology for the facts of correlation. So we may say in general, that the proper function of the congress is the establishment of common means of expressing the facts of geology. It should not meddle with the facts themselves. It may regulate the art of the geologist, but it must not attempt to regulate his science. Its proper field of work lies in the determination of questions of technology; it is a trespasser if it undertakes the determination of questions of science. It may decree terms, but it must not decree opinions.

ART. XLIX.—*On the Existence of certain Elements, together with the discovery of Platinum, in the sun. Contributions from the Physical Laboratory of Harvard University;* by C. C. HUTCHINS and E. L. HOLDEN.

[From the Proceedings of the American Academy of Arts and Sciences, vol. xxiii.]

LATE in the fall of 1886 it was decided by the writers, who were then at work in the Physical Laboratory of Harvard University, to attempt a revision of some of the previous work in regard to the chemical constitution of the sun, as well as to discover, if possible, new facts bearing on the same subject. For the purpose of this investigation a magnificent diffraction grating, made by Professor Rowland of Baltimore, was kindly

placed at our disposal by Professor John Trowbridge, under whose supervision and directions the subsequent work has been done.

After some delay caused by the mounting of the grating and its attachments, work was begun early in January, 1887, but, owing to bad weather and other hindrances, was not regularly and systematically prosecuted till somewhat later.

The grating used is of speculum metal with a ruled surface measuring 6 inches by 2, having 14,438 lines to the inch. It is concave, its radius of curvature being 21½ feet, and is mounted according to Professor Rowland's method. Suffice it to say, that the method is such that, by simply rolling the camera along an iron track, it passes not only from one part of the spectrum to another, but also to the spectra of different orders, at the will of the operator. As the distances on this track are proportional to the relative wave-lengths of the lines that fall successively on a given point in the camera, it is easy, by means of a suitable scale of equal parts placed beside the track, to set the center of the photographic plate instantly within a single wave-length of any given line in the spectrum.

And here let us parenthetically state that all our wave-lengths are those given by Professor Rowland's photographic map of the solar spectrum, the position of every line referred to being carefully identified upon the map, and its absolute wave-length thus determined. Although some of the negatives contain many lines too faint to show on the map, yet we feel confident that our numbers correspond in all cases to those of the map within one-tenth of a wave-length.

The light is brought into the room by means of a *porte lumière* and then sent through the slit after total reflection by a right-angled prism. Before striking the prism it passes through a cylindrical lens, which condenses it to a band of light about 2 inches long and ⅛ inch wide. The jaws of the slit move equally in opposite directions, so that, however widely they may be opened, no lateral displacement of lines can result from this cause.

Directly in front of the slit is placed a large tin lantern containing an electric lamp; the image of the arc can be brought exactly upon the slit by means of an adjustable lens in the front of the lantern. In the lower carbon of the lamp is made a cup-shaped cavity, which is filled with the substance a spectrum of which is desired. It is not at all necessary that this be in the form of a metal, for any ordinary compound is at once reduced by the intense heat and the presence of carbon vapor to the metallic state.

The plan of working has been as follows. The apparatus being arranged as described, the sunlight is admitted and the

desired portion of solar spectrum photographed upon the upper half of the plate; then the sunlight is excluded by a shutter, and the image of the electric arc containing the proper metal is allowed to fall upon the slit, and its spectrum photographed on the lower half of the plate. (Most of the plates used were those made by the M. A. Seed Co., and were cut to the size of 8 inches by 2. The most sensitive plates were obtained, and even then we found the required time of exposure for some parts of the spectrum inconveniently long.)

In order to effect the exposure of either half of the plate at will, we placed directly in front of the camera an opaque screen, in which was a rectangular opening one half the size of the plate. By turning a handle, this screen is raised or lowered without the slightest disturbance of camera or plate. The metallic spectrum, being thus photographed immediately below the solar spectrum, can be compared with it at leisure.

These spectra are then examined with the aid of a glass magnifying about ten diameters, and any concidences between solar and metallic lines carefully noted according to their wavelengths. In order to eliminate any personal error, they are examined by both observers separately, and their results afterwards compared.

To eliminate errors arising from suspected impurites of materials, as also from the impurities known to exist in the carbons employed, we took what we called "comparison photographs." For these, we placed in the carbon cup a portion of the substances known or suspected to be present as impurities in our metal, and then photographed the spectrum thus given on the upper half of the plate; a piece of the metal under experiment was then placed in the lamp, and the spectrum photographed on the lower part of the plate. Any lines due to impurities would then extend entirely across the plate, while those of the pure metal would extend only half way. In addition to this precaution we consulted all accessible tables and plates as to the position of known lines of metallic spectra, and also compared together all our photographs of the same region. If all of these tests left any doubt as to the origin of a given line, it was at once subjected to special investigation until all doubt was removed.

The dispersion given by the apparatus in the order of spectrum in which we worked is such that a single wave-length occupies on the negative a space of 1·12 mm. This makes the distance between the lines D_1 and D_2 6·7 mm., while the length of spectrum from A to H is about 4·1 m. With so great dispersion it would hardly be possible to mistake the position of a line by any very considerable amount, or to confound neighboring lines belonging to different metals.

For reasons readily apparent, it was found so difficult to photograph under high dispersive power those parts of the spectrum not lying between wave-length 3600 and wave-length 5000, that our photographic work was done chiefly within those limits. It was, however, supplemented in many cases by eye observations in other portions of the spectrum.

We are convinced that there is much in the whole matter of coincidences of metallic and solar lines that needs re-examination; that something more than the mere coincidence of two or three lines out of many is necessary to establish even the probability of the presence of a metal in the sun. With the best instruments the violet portion of the solar spectrum is found to be so thickly set with fine lines, that, if a metallic line were projected upon it at random, in many places the chances for a coincidence would be even, and coincidences could not fail to occur in case of such metals as cerium and vanadium which give hundreds of lines in the arc.

Moreover, a high dispersion shows that very few lines of metals are simple and short, but, on the contrary, winged and nebulous, and complicated by a great variety of reversal phenomena. A "line" is sometimes half an inch wide on the photographic plate, or it may be split into ten by reversals.

At first, we believed that these reversals were due to defects in the ruling of the grating, but we are convinced that they are true phenomena from the following experiments. 1st. The wings continue when various portions of the grating are covered. 2d. They are the same in three successive orders of spectra. 3d. They are very different in different metals, and in some are not seen at all. 4th. We arranged a flat grating, with collimator and projecting lens, each of five feet focus, and found that with this apparatus the same phenomena appeared.

On pages 87 and 88 of "The Sun," Professor Young gives a list of elements in the sun according to the best authorities, which is followed by a list of doubtful elements. Some of these we have examined with the following results:

Cadmium.—The coincidence of the two lines given by Lockyer at wave-lengths 4677 and 4799 is perfect. These are the only cadmium lines near, and the sun lines in the vicinity are not numerous.

Lead.—The evidence for lead, due to Lockyer, is based upon three lines at 4019·7, 4058·2 and 4061·8. We have photographed these lines with the sun many times. They are broad and nebulous, and often several times reversed. Lines in solar spectrum numerous and faint. 4019·7 and 4058·2 certainly do not coincide. 4061·8 is very difficult to pronounce upon; it may coincide.

Cerium, *Molybdenum*, *Uranium* and *Vanadium.*—These four

metals may be classed together. Lockyer finds four coincidences each for molybdenum and vanadium, three for uranium, and two for cerium. The arc spectrum of each is characterized by great complexity and vast numbers of lines. So numerous are the lines in fact, that often on the photographs the total space occupied by them is greater than the space not so occupied. A plate ten inches long may contain a thousand or so. Evidently coincidences between these and solar lines cannot fail to occur as matters of chance, and therefore prove nothing. One can easily count a hundred or so such coincidences without the slightest conviction that the connection is other than fortuitous. Of course all this is nothing against the probability of these metals being in the sun; but at the same time those peculiarities of grouping, strength of lines, and other characteristics which occur in the case of iron and other spectra, and which alone can serve as evidence in such cases, are conspicuously absent.

Among the metals whose existence in the solar atmosphere has seemed probable, we have examined the following:

Bismuth.—The line of the above metal at 4722·9, the only line of bismuth in the arc in that whole region, coincides perfectly with the more refrangible of a very faint pair of solar lines.

Tin.—The solitary tin line at 4525, thought by Lockyer to coincide, falls directly between two fine lines in the solar spectrum.

Silver.—Lockyer mentions a certain possibility of silver in the solar atmosphere from the apparent agreement of two of its nebulous lines with solar lines. One of these we have never been able to find in the course of many photographs of the region in which it is given by him.

We find seven lines of silver between 4000 and 4900. Of these seven, three are what Thalen calls nebulous; so broad and hazy that their true positions cannot be determined with much accuracy. These lie at about 4055·5, 4063 6, and 4212. A fourth line at 4023 is of the same general character, but has a sharp reversal which agrees with a solar line. The remaining three lines are represented in the sun, and are given by Thalen in the spark spectrum of the metal.

4476·2. Very strong line; nebulous on lower edge. Sun line strong. (Thalen, 4475.)

4668·8. Strong, solitary line. (Thalen, 4666·5.)

4874·3. Fairly strong. (Thalen, 4874.)

Thus, between the limits given above, every line of silver, as far as can be determined, coincides with a solar line.

Potassium.—We could find but two lines of potassium, the same that were examined by Lockyer, 4044·5 and 4048·35.

Each line is reversed four times, which increases the difficulty of locating them exactly. 4048·35 seems to agree with a solar line. The solar line near 4044·5 is very faint, and it is next to impossible to decide the question of an agreement.

Lithium.—The blue line of lithium presents a curious case. The very broad and nebulous line has a rather sharp reversal near the center, and somewhat toward the lower edge a broader and less clearly defined reversal. Both these reversals agree with solar lines at 4602·5 and 4603·2. It is possible that one of the reversals may be due to the presence of some other substance, say calcium; but if that were true, it would seem that both reversals would be nearly, if not quite, obliterated. Further experiment may clear the matter up. 4603·2 is given to iron by Thalen.

Platinum.—As far as we can learn, no evidence has hitherto been offered to show the occurrence of this metal in the solar atmosphere. We were somewhat surprised, therefore, upon meeting with coincidences. Between 4250 and 4950 we find 64 lines of platinum, sixteen of which agree with solar lines. The latter are at the following places:—

4291·10	4481·85
4392·00 (Thalen 4389·4)	4552·80 (Thalen 4551·8)
4430·40	4560·30
4435·20	4580·80
4440·70	4852·90 (Thalen 4851·5)
4445·75 (Thalen 4442·0)	4857·70
4448·05	4899·00
4455·00	4932·40

We have taken all possible care to make this statement accurate, and to admit no lines about which there seems to be any question. There are seven other lines not included in the list, the probability of agreement of which is at least as good as that upon which potassium is admitted.

In all these experiments everything has been done to bring out and show upon the photograph as much as possible. The lamp, constructed for the purpose and fed by a powerful dynamo, gave an arc from a half to three-fourths of an inch long, and with a long flame and so intense a heat that it could be worked for but a few minutes at a time. Any one who has carried out a series of experiments like this is alone competent to appreciate the great labor and the endless difficulties and perplexities that attend them.

Our thanks are especially due to Dr. Wolcott Gibbs for his hearty encouragement, and for the use of valuable apparatus and chemicals.

ART. L.—*The Flora of the Coast Islands of California in relation to recent changes of Physical Geography;* by JOSEPH LECONTE.

SOME of the results reached by Mr. E. L. Greene in his studies of the flora of the islands off the coast of Southern California* have deeply interested me, because I believe their explanation may be found in geologically recent changes in the physical geography of California.

These remarkable islands, 8 or 10 in number, are strung along the coast from Point Conception southward, and separated from the mainland by a sound 20–30 miles wide. They are of considerable size (the largest being about 200 square miles in extent), and vary in height from 1,000 to 3,000 feet. They have all the characteristics of continental islands, and are undoubtedly outliers of the mainland, at one time connected with it, but now separated by subsidence of the continental margin. They may be regarded as the highest points of an old coast range outside of the present coast range, the broad valley between the two being now covered with water. Moreover, the date of the separation may be determined with certainty. That they were connected with the mainland during the later Pliocene and early Quaternary is proved by the fact that remains of the mammoth have been found on Santa Rosa, the largest and one of the farthest off of them.† *They were, therefore, undoubtedly separated during the Quaternary Period.*

The main points in Mr. Greene's paper with which we are here concerned are the following

1. Out of 296 species of plants collected by him on the island of Santa Cruz, no less than 48 are entirely peculiar to these islands, and 28 peculiar to Santa Cruz itself.

2. Of the remaining 248 species nearly all are *distinctively Californian*—that is, species peculiar to California are very abundant, while common American species, *i. e.*, those common to California and other parts of North America, are very few and rare. The flora as a whole, therefore, may be regarded as distinctively Californian, with the addition of a large number of species wholly peculiar to the islands.

3. A number of rare species found in isolated patches, and, as it were, struggling for existence, in the southern counties—San Diego and San Bernardino—are found in *great abundance and very thriving condition on the islands.*

* Studies in the Botany of California and Parts Adjacent. VI. E. L. Greene. I—Notes on Botany of Santa Cruz Island. Bull. 7, Cal. Acad. Sci.

† Proc. Cal. Acad. Sci., vol. v, 152.

4. *Lavatera*, a remarkable malvaceous genus of which 18 species are known in the Mediterranean region, and one from Australia, but *not a single species on the American Continent, is represented on these islands by four species.* This is certainly a most remarkable and significant fact.

Such are the facts. I account for them as follows:

California, especially the region west of the Sierra Nevada, is geologically very recent. The Sierra region was reclaimed from the sea at the beginning of the Cretaceous, and the coast region as late as the beginning of the Pliocene. When first emerged the coast region was of course colonized from adjacent parts. This colonization was probably mainly from Mexico, either directly or through the Sierra region; for the distinctively Californian plants, though peculiar, are more like those of Mexico than any other. Whencesoever it may have been colonized, however, the environment was sufficiently peculiar, the isolation sufficiently complete, and the time has been sufficiently long to make a very distinct flora. According to Wallace it is one of the primary divisions of the Nearctic Region.

During the late Pliocene and early Quaternary, as already seen, the islands were still a part of the mainland, and the whole was occupied by the same flora, viz: the distinctively Californian (with some differences doubtless), now found in both, together with the peculiar island-species.

During the oscillations of the Quaternary the then westernmost coast range was separated by subsidence, and has remained ever since as islands. Simultaneously with, or after, this separation, came the invasion of northern species, driven southward by glacial cold. Then followed the mingling of invaders with the natives, the struggle for mastery, the extermination of many (viz: the peculiar island species), and perhaps the slight modification of all, and the final result is the California flora of today. But the island flora was saved from this invasion by isolation, and therefore far less changed than the flora of the mainland, *i. e.*, the invading species are mostly wanting, and many species survived there which were destroyed, or else modified into other species, on the mainland, and the remainder probably less modified than on the mainland. The flora of these islands, therefore, represents somewhat nearly the character of the flora of the whole country during the Pliocene times. Some modification they have doubtless suffered, but the time has been too short for any great change in the absence of severe competition.

The question naturally arises, "How is it that with a separation of only 20–30 miles the two floras—insular and mainland—have not become entirely similar by mutual colonization?" The prevailing winds being landward would, I sup-

pose, largely prevent the colonization of common American forms on the islands, although some such colonization has in fact taken place. But with the prevailing winds in this direction, why have not all the peculiar island species been long ago colonized on the mainland? According to the view above presented the answer is evident. These peculiar species did once inhabit the mainland, and have been either destroyed or transformed by the struggle with invaders. They are therefore *weaker* species. The same unfitness which made them succumb then, still forbids their successful colonization. This brings me to the next point.

There are quite a number of rare and peculiar forms found struggling for existence in the southern counties which are found very abundant on the islands. This certainly looks like the beginning of colonization. This is indeed Mr. Greene's view, and is rendered all the more probable by the fact that the ocean currents probably drift in that direction. But there is at least another explanation suggested by the view above presented. These may be, and probably are, *remnants* of Pliocene indigenes still undestroyed, but ready to perish. From this point of view their place far south is just what we might expect, for the main invasion was from the north.

But there is still a last point to be explained. *Lavateras* are unknown in the New World, except on these islands, where there are four species. But they are found in the Old World, in the Mediterranean region and in Australia. Mr. Greene suggests, as a possible explanation, *a former connection of these islands with some other continent.* I think not. The substantial permance of continental land masses and oceanic basins, with only marginal changes, at least during later geological times—taken together with the comparative recency of the flora of California—renders this explanation extremely improbable. The above presented view suggests another and far more probable explanation.

The existence of *Lavateras* in such widely separated localities as Australia, the Mediterranean region and the coast islands of California, shows unmistakably that existing species are but remnants of an old, once very abundant and widely spread genus, with numerous species. They are now dying out. They have been mostly destroyed and replaced by newer and stronger forms. I conclude, therefore, that in Pliocene times several species of *Lavatera* existed all over the coast region of California, but probably mostly in the then coast range, viz: the islands; for they love the sea coast. They have all been destroyed by change of environment, physical and organic, except those isolated on the islands and thus saved from the effects of invasion.

Readers of Mr. Wallace's "Island Life" will at once see the analogy between this explanation of the flora of our coast islands and Mr. Wallace's explanation of the mammalian fauna of Madagascar. The mammalian fauna of Africa, south of Sahara, consists of two very distinct groups—the one *indigenous* or descendants of Tertiary indigenes, and *remotely* resembling that of Madagascar, the other evidently *foreign* and resembling that of *Eurasia in Miocene and Pliocene times.* During Tertiary times Africa was isolated from Eurasia, but united with Madagascar, and the whole inhabited by a peculiar fauna, characterized by lemurs, insectivores, etc., which we have called indigenes. About middle Tertiary times, Madagascar was separated, and immediately divergence between the two faunas commenced. In the later Tertiary and early Quaternary, the barrier which separated Africa from Eurasia was removed, and the great Eurasian animals invaded Africa, and immediately became the dominant type. In the struggle which ensued, many species, especially of the weaker indigenes, were destroyed, and all on both sides modified. The result is the African fauna of to-day. Madagascar was saved from this invasion by isolation. The fauna there consists of the greatly modified descendants of the African Tertiary indigenes, but far less modified than their congeners in Africa. In the fauna of Madagascar, therefore, we have the nearest approach to the Tertiary indigenes of both.

The difference between the two cases is this: In the case of Madagascar the separation has been very long. The extreme peculiarity of its fauna is the result partly of progressive *divergence* and partly of *many forms saved by isolation.* In the case of the coast islands of California, the separation is comparatively recent—there has not been time enough for very great divergence by modification. The peculiarity of its flora is due almost wholly to species saved by isolation.

In conclusion I would say, that this short paper is intended merely as an incentive to future investigation and pointing in the direction which it ought to take. Before the views above presented can be definitely established, there must be further investigations, first, on the relation of the island flora to that of the mainland; second, on the relation of the flora of California to that of adjacent parts from which it may have been originally colonized; third, and especially, must we have fuller knowledge of the indigenous flora of California in Pliocene times.

ART. LI.—*Determination of "prevailing wind direction;"* by H. ALLEN HAZEN.

VARIOUS methods have been adopted for the graphic representation of the prevailing wind direction. Perhaps the simplest of these consists in drawing radial lines for each of the eight principal points of the compass and making the length of each line correspond to the number of times the wind has blown from that direction. A broken line joining the ends of these would enclose an area, and the portions of this area between the radials would indicate approximately the frequency of the wind. If the winds were not of the same velocity from all points, this could be indicated by figures on each line denoting the mean velocity from each direction. The difference would be inappreciable in the United States, and in fact wherever there is a difference the velocity will be greater in the line of the prevailing direction, so that, allowing for velocity, we would simply intensify the mean direction. In no case would there be any error in the mean direction ensuing from a neglect of a slight variation in the velocity.

Lambert's formula.

Perhaps the best method of getting this quantity is by means of Lambert's formula. The ordinary computation of this formula is exceedingly tedious, especially where several thousand computations are to be made. I have prepared very simple tables, for my own use, which do away with the use of logarithms and enable any one at a glance to take out the quantity sought. The whole process is a simple mechanical one and a child can perform the work without difficulty. It has seemed wise to prepare these tables for wider use, because, as the science of meteorology advances the demand is for increased accuracy in its results. The examination of any monthly mean chart with wind directions accurately projected upon it will show distinctly the usual law of circulation of winds about high and low pressures, and where the winds do not seem to follow this law it can be inferred that there is some local peculiarity which affects the wind. These tables are specially commended to voluntary observers of the weather for the computation of their mean wind direction.

We have Lambert's formula as follows:

$$\tan A = \frac{E-W+(NE-SW)\cos 45^\circ+(SE-NW)\cos 45^\circ}{N-S+(NE-SW)\cos 45^\circ-(SE-NW)\cos 45^\circ}$$

In which N, NE, E, etc., represent the number of times the wind has been observed from that direction during the period

under discussion. The simplest method of application would be to prepare a form in which to enter the observations in an order most suited for computation. Strict attention must be paid throughout to the algebraic signs. To those who are unaccustomed to algebraic processes the following rules will be well worth remembering. For algebraic addition — if the signs of the quantities are *alike* add them; if *unlike* subtract them and give the result the sign of the larger quantity, e. g. $3+4=7$, $-16+4=-12$. For subtraction—change the sign of the second quantity and proceed as in addition, e. g. $(+3)-(+4)=-1$, $(-16)-(+4)=-20$. The form to be used is as follows:

a	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*i*	*k*	*l* $\times$ cos 45°	*m* $\times$ cos 45°	*n*	*o*	*p*	*q*	*r*	*s*		Direction.
E	W	N	S	NE	SW	SE	NW	NE-SW	SE-NW			E-W	*l*+*m*	N-S	*l*−*m*	*o*+*n*	*p*+*q*	$r \div s$	
2	12	21	26	13	9	0	10	4	−10	2·8	−7·1	−10	−4·3	−5·0	9·9	−14·3	4·9	19	N 71° W

Fill in the columns *a* to *h* with the number of times the wind has blown from the eight points of the compass. The remaining computations are indicated at the head of each column. The values in (*l*) and (*m*), *i* and *k*, multiplied by cos 45°, may be taken from Table I. Having determined *r* and *s*, we have reduced the winds from the eight points to two at right angles to each other, or we have a right-angled triangle in which we have given the base and perpendicular, the tangent of the true angle of the wind being the one divided by the other. Winds from N and E are regarded as positive +. The quadrant in which the angle lies is obtained as follows: $\frac{-r}{+s}$ = NW : $\frac{+r}{-s}$ = SE : $\frac{+r}{+s}$ = NE : $\frac{-r}{-s}$ = SW. In writing the angle, N. or S. should *always* be placed first. The value of the angle may be immediately taken out of the table "Values of A" when we have *r* and *s* given. If either or both of these be greater than 160, divide both by such a number as will make the larger, 160 or less. Find the smaller number in the top horizontal row of figures, the other in the vertical row at the left. The number at the intersection of the two will be the angle sought, unless *s* is smaller than *r*. If so, subtract the number from 90°. In the example given, $r=-14{\cdot}3$; $s=4{\cdot}9$ at the intersection of 49 and 145 we find 19°; since *s* is the smaller we must subtract 19° from 90° and we have $\frac{-r}{+s}$ = N 71° W. It will be readily seen that in entering the final table we may ignore the decimal points, although with a good deal more trouble we could have gotten the same result by entering with 4·9

and 14·3. If we should have 5 and 14·3 it would be much better to multiply the whole number by 10 and enter the table with 50 and 143, i. e., the larger the numbers used, provided they are within the limits of the table the easier the computation.

The instructions in this paper have been prepared by Mr. R. A. Clark.

June 7, 1887.

TABLE I.—*Multiples of cosine* 45° (·7071).

Tens.	Units. 0	1	2	3	4	5	6	7	8	9	Tens.
0	0·0	·7	1·4	2·1	2·8	3·5	4·2	4·9	5·7	6·4	0
10	7·1	7·8	8·5	9·2	9·9	10·6	11·3	12·0	12·7	13·4	10
20	14·1	14·8	15·6	16·3	17·0	17·7	18·4	19·1	19·8	20·5	20
30	21·2	21·9	22·6	23·3	24·0	24·7	25·5	26·2	26·9	27·6	30
40	28·3	29·0	29·7	30·4	31·1	31·8	32·5	33·2	33·9	34·6	40
50	35·4	36·1	36·8	37·5	38·2	38·9	39·6	40·3	41·0	41·7	50
60	42·4	43·1	43·8	44·5	45·3	46·0	46·7	47·4	48·1	48·8	60
70	49·5	50·2	50·9	51·6	52·3	53·0	53·7	54·4	55·2	55·9	70
80	56·6	57·3	58·0	58·7	59·4	60·1	60·8	61·5	62·2	62·9	80
90	63·6	64·3	65·1	65·8	66·5	67·2	67·9	68·6	69·3	70·0	90
100	70·7	71·4	72·1	72·8	73·5	74·2	75·0	75·7	76·4	77·1	100
110	77·8	78·5	79·2	79·9	80·6	81·3	82·0	82·7	83·4	84·1	110
120	84·9	85·6	86·3	87·0	87·7	88·4	89·1	89·8	90·5	91·2	120
130	91·9	92·6	93·3	94·0	94·8	95·5	96·2	96·9	97·6	98·3	130
140	99·0	99·7	100·4	101·1	101·8	102·5	103·2	103·9	104·7	105·4	140
150	106·1	106·8	107·5	108·2	108 9	109·6	110·3	111·0	111·7	112·4	150

In using this table find the tens and hundred of the number to be multiplied by cos 45° in the left hand column and the units in the upper horizontal line. The product will be found at the intersection of the horizontal and vertical lines from these. For example, 148 × cos 45° at the intersection of 140 and 8 we find 104·7 answer.

Values of the angle A.

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50
10	6	11	17	22	27	31	35	39	42	45																																									
15	4	8	11	15	18	22	25	28	31	34	36	39	41	43	45																																				
20	3	6	9	11	14	17	19	22	24	27	29	31	33	35	37	39	40	42	44	45																														
25	2	5	7	9	11	13	16	18	20	22	24	26	27	29	31	33	34	36	37	39	40	41	43	44	45																										
30	2	4	6	8	9	11	13	15	16	18	20	22	23	25	27	28	30	31	32	34	35	36	37	39	40	41	42	43	44	45																				
35	2	3	5	7	8	10	11	13	14	16	17	19	20	22	23	25	26	27	28	30	31	32	33	34	36	37	38	39	40	41	42	43	44	45																
40	1	3	4	6	7	9	10	11	13	14	15	17	18	19	21	22	23	24	25	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	44	45											
45	1	3	4	5	6	8	9	10	11	13	14	15	16	17	18	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	39	40	41	42	42	43	44	44	45						
50	1	2	3	5	6	7	8	9	10	11	12	13	15	16	17	18	19	20	21	22	23	24	25	26	27	27	28	29	30	31	32	33	33	34	35	36	37	37	38	39	39	40	41	41	42	43	43	44	44	45
55	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	24	25	26	27	28	29	29	30	31	32	32	33	34	35	35	36	37	37	38	39	39	40	41	42	42	43
60	1	2	3	4	5	6	7	8	9	9	10	11	12	13	14	15	16	17	18	18	19	20	21	22	23	23	24	25	26	27	27	28	29	30	30	31	32	32	33	34	34	35	36	36	37	37	38	39	39	40
65	1	2	3	4	4	5	6	7	8	9	10	10	11	12	13	14	15	15	16	17	18	19	19	20	21	22	23	23	24	25	25	26	27	28	28	29	30	30	31	32	32	33	33	34	35	35	36	36	37	38
70	1	2	2	3	4	5	6	7	7	8	9	10	11	11	12	13	14	14	15	16	17	17	18	19	20	20	21	22	23	23	24	25	25	26	27	27	28	28	29	30	30	31	32	32	33	33	34	34	35	36
75	1	2	2	3	4	5	5	6	7	8	8	9	10	11	11	12	13	13	14	15	16	16	17	18	18	19	20	20	21	22	22	23	24	24	25	26	26	27	27	28	29	29	30	30	31	32	32	33	33	34
80	1	1	2	3	4	4	5	6	6	7	8	9	9	10	11	11	12	13	13	14	15	15	16	17	17	18	19	19	20	21	21	22	22	23	24	24	25	25	26	27	27	28	28	29	29	30	30	31	31	32
85	1	1	2	3	3	4	5	5	6	7	7	8	9	9	10	11	11	12	13	13	14	14	15	16	16	17	18	18	19	19	20	21	21	22	22	23	24	24	25	25	26	26	27	27	28	28	29	29	30	30
90	1	1	2	3	3	4	4	5	6	6	7	8	8	8	9	9	10	11	11	12	13	13	14	14	15	16	16	17	17	18	18	19	20	20	21	21	22	22	23	23	24	25	26	26	27	27	28	28	29	29
95	1	1	2	2	3	4	4	5	5	6	7	7	8	8	9	10	10	11	11	12	12	13	14	14	15	15	16	16	17	18	18	19	19	20	20	21	21	22	22	23	24	24	25	25	26	26	27	27	28	28
100	1	1	2	2	3	3	4	5	5	6	6	7	7	8	8	9	10	10	11	11	12	12	13	13	14	15	15	16	16	17	17	18	18	19	19	20	20	21	21	22	22	23	23	24	24	25	25	26	26	27
105	1	1	2	2	3	3	4	4	5	5	6	7	7	8	8	9	9	10	10	11	11	12	12	13	13	14	14	15	15	16	16	17	17	18	18	19	19	20	20	21	21	22	22	23	23	24	24	25	25	25
110	1	1	2	2	3	3	4	4	5	5	6	6	7	7	8	8	9	9	10	10	11	11	12	12	13	13	14	14	15	15	16	16	17	17	18	18	19	19	20	20	21	21	22	22	23	23	24	24	24	24
115	0	1	1	2	2	3	3	4	5	5	6	6	7	7	8	8	9	9	10	10	11	11	12	12	13	13	14	14	15	15	16	16	17	17	18	18	19	19	20	20	21	21	22	22	23	23	23	23	23	23
120	0	1	1	2	2	3	3	4	4	5	5	6	6	7	7	8	8	9	9	9	10	10	11	11	12	12	13	13	14	14	15	15	16	16	17	17	18	18	19	19	20	20	21	21	22	22	23	23	23	23
125	0	1	1	2	2	3	3	4	4	5	5	6	6	7	7	7	8	8	9	9	10	10	10	11	11	12	12	13	13	13	14	14	15	15	16	16	16	17	17	18	18	19	19	19	20	20	21	21	21	22
130	0	1	1	2	2	3	3	4	4	4	5	5	6	6	7	7	7	8	8	9	9	10	10	10	11	11	12	12	13	13	13	14	14	15	15	15	16	16	17	17	18	18	18	19	19	19	20	20	21	21
135	0	1	1	2	2	3	3	3	4	4	5	5	5	6	6	7	7	8	8	8	9	9	10	10	10	11	11	12	12	13	13	13	14	14	15	15	15	16	16	17	17	17	18	18	18	19	19	20	20	20
140	0	1	1	2	2	2	3	3	4	4	4	5	5	6	6	6	7	7	8	8	8	9	9	10	10	11	11	11	12	12	12	13	13	14	14	14	15	15	16	16	16	17	17	17	18	18	19	19	19	20
145	0	1	1	2	2	2	3	3	3	4	4	4	5	5	5	6	6	6	7	7	8	8	8	9	9	9	10	10	11	11	11	11	12	12	12	13	13	13	14	14	14	15	15	15	16	16	16	17	17	17
150	0	1	1	1	2	2	3	3	3	4	4	4	5	5	5	6	6	6	7	7	8	8	8	9	9	9	10	10	10	11	11	11	12	12	12	13	13	13	14	14	14	15	15	15	16	16	16	17	17	17
155	0	1	1	1	2	2	2	3	3	3	4	4	4	5	5	5	6	6	7	7	8	8	8	9	9	10	10	10	11	11	11	12	12	12	13	13	13	14	14	14	15	15	15	16	16	16	17	17	17	18
160	0	1	1	1	2	2	2	3	3	3	3	4	4	4	5	5	5	6	6	7	7	8	8	8	9	9	9	10	10	10	10	11	11	11	12	12	12	13	13	13	14	14	14	15	15	15	16	16	16	17
165	0	1	1	1	2	2	2	3	3	3	3	4	4	4	5	5	5	6	6	6	7	7	7	8	8	8	9	9	9	10	10	10	11	11	11	12	12	12	13	13	13	14	14	14	15	15	15	16	16	16
170	0	1	1	1	2	2	2	3	3	3	3	4	4	4	5	5	5	5	6	6	6	7	7	7	8	8	8	9	9	9	9	10	10	10	11	11	11	11	12	12	12	13	13	13	14	14	14	15	15	16
175	0	1	1	1	2	2	2	3	3	3	3	4	4	4	5	5	5	5	6	6	6	7	7	7	8	8	8	8	9	9	9	9	10	10	10	11	11	11	11	12	12	12	13	13	13	14	14	14	15	15
180	0	1	1	1	1	2	2	2	3	3	3	3	4	4	4	5	5	5	6	6	6	7	7	7	8	8	8	9	9	9	9	10	10	10	11	11	11	11	12	12	12	13	13	13	14	14	14	15	15	16
185	0	1	1	1	1	2	2	2	3	3	3	3	4	4	4	4	5	5	5	6	6	6	6	7	7	7	8	8	8	8	9	9	9	9	10	10	10	10	11	11	11	11	12	12	12	12	13	13	13	14
190	0	1	1	1	1	2	2	2	2	3	3	3	4	4	4	4	5	5	5	5	6	6	6	6	7	7	7	7	8	8	8	8	9	9	9	9	10	10	10	10	11	11	11	11	12	12	12	13	13	13
195	0			1	1	1	2	2	2	3	3	3	4	4	4	4	4	5	5	5	6	6	6	6	7	7	7	7	8	8	8	8	9	9	9	9	10	10	10	10	11	11	11	11	12	12	12	12	13	13	13
200	0	1	1	1	1	2	2	2	2	3	3	3	3	4	4	4	4	5	5	5	5	6	6	6	6	7	7	7	7	8	8	8	8	9	9	9	9	10	10	10	10	11	11	11	11	12	12	12	12	13
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50

Values of the angle A—continued.

	1 2 3 4 5	6 7 8 9 10	11 12 13 14 15	16 17 18 19 20	21 22 23 24 25	26 27 28 29 30	31 32 33 34 35	36 37 38 39 40	41 42 43 44 45	46 47 48 49 50

	50 52 54 56 58	60 62 64 66 68	70 72 74 76 78	80 82 84 86 88	90 92 94 96 98	100	1 1 1 1 02 04 06 08	1 1 1 1 1 10 12 14 16 18	1 1 1 1 1 1 20 22 24 26 28	1 1 1 1 1 1 30 32 34 36 38	1 1 1 1 1 1 40 42 44 46 48	1 1 1 1 1 1 50 52 54 56 58	1 60
55	42 43 44												
60	40 41 42 43 44	45											
65	38 39 40 41 42	43 44 45											
70	36 37 38 39 40	41 42 43 44	45										
75	34 35 36 37 38	39 40 40 41 42	43 44 45										
80	32 33 34 35 36	37 38 39 39 40	41 42 43 44 44	45									
85	30 31 32 33 34	35 36 37 38 39	40 40 41 42 43	43 44 44 45									
90	29 30 31 32 33	34 35 35 36 37	38 39 39 40 41	42 42 43 44 44	45								
95	28 29 30 31 31	32 33 34 35 36	36 37 38 39 39	40 41 41 42 43	43 44 45								
100	27 27 28 29 30	31 32 33 33 34	35 36 37 37 38	39 39 40 41 41	42 43 43 44 44	45							
105	25 26 27 28 29	30 31 31 32 33	34 34 35 36 37	37 38 39 39 40	41 41 42 42 43	44 44 45							
110	24 25 26 27 28	29 29 30 31 32	33 33 34 35 35	36 37 37 38 39	39 40 41 41 42	42 43 43 44 44	45						
115	23 24 25 26 27	28 29 29 30 31	31 32 33 33 34	35 36 36 37 37	38 39 39 40 40	41 42 42 43 43	44 44 45						
120	23 23 24 25 26	27 27 28 29 30	30 31 32 32 33	34 34 35 36 36	37 37 38 39 39	40 40 41 41 42	42 43 44 44 45	45					
125	22 23 23 24 25	26 26 27 28 29	29 30 31 31 31	33 33 34 35 35	36 36 37 37 38	39 39 40 40 41	41 42 42 43 43	44 44 45					
130	21 22 23 23 24	25 25 26 27 28	28 29 30 30 30	32 32 33 33 34	35 35 36 36 37	38 38 39 39 40	40 41 41 42 42	43 43 44 44 45	45				
135	20 21 22 23 23	24 25 25 26 27	27 28 29 29 29	31 31 32 32 33	34 34 35 35 36	37 37 38 38 39	39 40 40 41 41	42 42 43 43 43	44 44 45				
140	20 20 21 22 23	23 24 25 25 26	27 27 28 28 28	30 30 31 32 32	33 33 34 34 35	36 36 37 37 38	38 39 39 40 40	41 41 42 42 42	43 43 44 44 45	45			
145	19 20 20 21 22	22 23 24 24 25	26 26 27 28 27	29 30 30 31 31	32 32 33 33 34	35 35 36 36 37	37 38 38 39 39	40 40 41 41 41	42 42 43 43 44	44 44 45			
150	18 19 20 20 21	22 22 23 24 24	25 26 26 27 27	28 29 29 30 30	31 32 32 33 33	34 34 35 35 36	36 37 37 38 38	39 39 40 40 40	41 41 42 42 43	43 43 44 44 45	45		
155	18 19 19 20 20	21 22 22 23 24	24 25 25 26 26	27 28 28 29 30	30 31 31 32 32	33 33 34 34 35	35 36 36 37 37	38 38 39 39 40	40 40 41 41 42	42 42 43 43 44	44 44 45		
160	17 18 19 19 20	21 21 22 22 23	24 24 25 25 26	27 27 28 28 29	29 30 30 31 31	32 33 33 34 34	35 35 35 36 36	37 37 38 38 39	39 39 40 40 41	41 42 42 42 43	43 44 44 44 45	45	
165	17 18 18 19 19	20 21 21 22 22	23 24 24 25 25	26 26 27 27 28	29 29 30 30 31	31 32 32 33 33	34 34 34 35 35	36 36 37 37 38	38 39 39 39 40	40 41 41 42 42	42 43 43 44 44	44	
170	16 17 18 18 19	19 20 21 21 22	22 23 24 24 25	25 26 26 27 27	28 28 29 29 30	30 31 31 32 32	33 33 34 34 35	35 36 36 37 37	37 38 38 39 39	39 40 40 41 41	41 42 42 43 43	43	
175	16 17 17 18 18	19 19 20 20 21	22 22 23 23 24	25 25 26 26 27	27 28 28 29 29	30 30 31 31 31	32 32 33 33 34	34 35 35 36 36	37 37 38 38 38	39 39 40 40 40	41 41 41 42 42	42	
180	16 16 17 17 18	18 19 20 20 21	21 22 22 23 23	24 24 25 26 26	27 27 28 28 29	29 30 30 30 31	31 32 32 33 33	34 34 35 35 35	36 36 37 37 37	38 38 39 39 39	40 40 41 41 41	42	
185	15 16 16 17 17	18 19 19 20 20	21 21 22 22 23	23 24 24 25 25	26 26 27 28 28	28 29 29 30 30	31 31 32 32 33	33 33 34 34 35	35 35 36 36 37	37 37 38 38 39	39 40 40 40 41	41	
190	15 15 16 16 17	18 18 19 19 20	20 21 21 22 22	23 23 24 24 25	25 26 26 27 27	28 28 29 29 30	30 31 31 31 32	32 33 33 34 34	34 35 35 36 36	36 37 37 38 38	38 39 39 39 40	40	
195	14 15 15 16 17	17 18 18 19 19	20 20 21 21 22	22 23 23 24 24	25 25 26 26 27	27 28 28 29 29	29 30 30 31 31	32 32 33 33 33	34 34 35 35 35	36 36 36 37 37	38 38 38 39 39	39	
200	14 15 15 16 16	17 17 18 18 19	19 20 20 21 21	22 22 23 23 24	24 25 25 26 26	27 27 27 28 28	29 29 30 30 31	31 32 32 32 33	33 33 34 34 35	35 35 36 36 37	37 37 38 38 38	39	

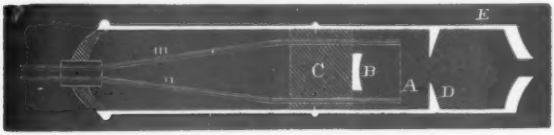
	50 52 54 56 58	60 62 64 66 68	70 72 74 76 78	80 82 84 86 88	90 92 94 96 98	100	1 1 1 1 02 04 06 08	1 1 1 1 1 1 10 12 14 16 18	1 1 1 1 1 1 20 22 24 26 28	1 1 1 1 1 1 30 32 34 36 38	1 1 1 1 1 1 40 42 44 46 48	1 1 1 1 1 1 50 52 54 56 58	1 60

ART. LII.—*A new instrument for the measurement of Radiation;* by C. C. HUTCHINS.

THE difficulties which attend the use of the thermo-pile as an accurate measurer of radiations, are familiar to all who have had any experience with that instrument. The slowness of its indications, and the long time required for it to return to zero, are defects which entirely unfit it for many delicate experiments.

It occurred to the writer that sensitiveness to radiation might as well be secured by employing a very thin thermal-junction with some condensing arrangement, as by the use of several pairs of stout bars, as in the ordinary thermo-pile; for the thin junction would be heated to a much higher temperature than a thick one by a given quantity of heat, and have the great advantage of quickly parting with its heat and returning to the temperature of the surrounding atmosphere.

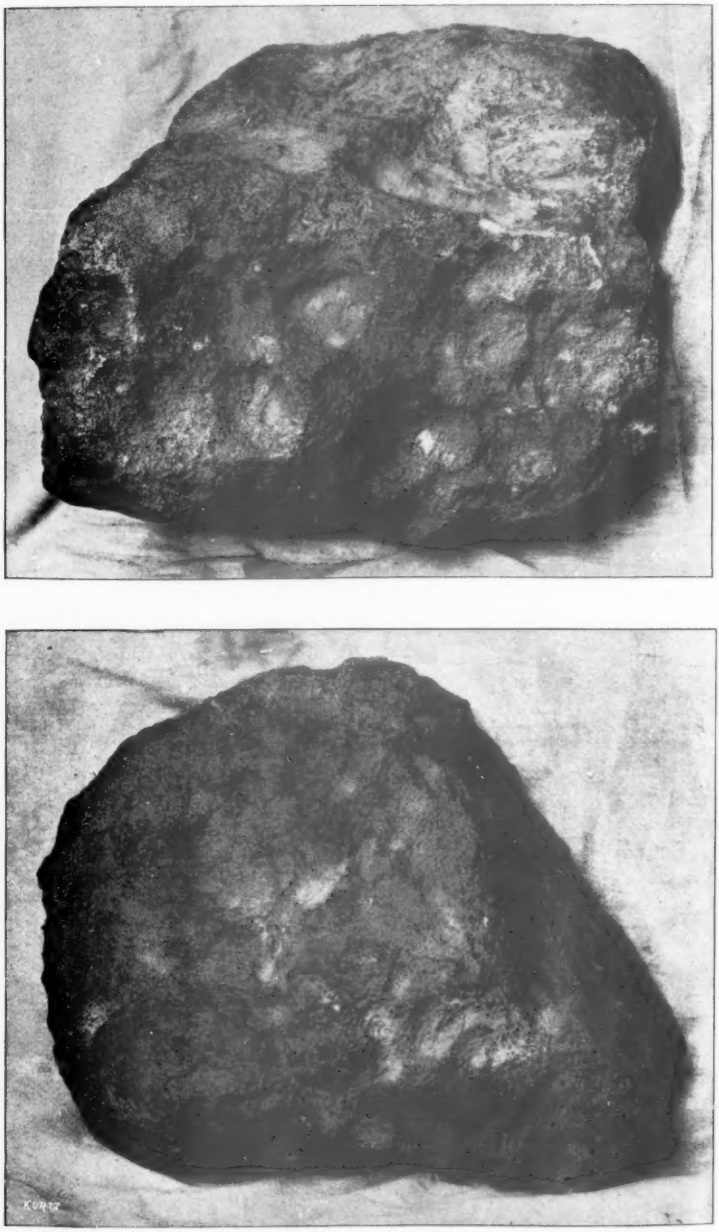
The instrument is constructed upon these principles as follows: A tube of vulcanite ten inches long, two and a half inches in diameter, is stopped near the middle by a plug of wood. The tube is made separable, and this plug serves to unite its two halves as well as to support the working parts. Through the plug pass two small copper rods projecting about an inch above the plug toward the front of the instrument, and passing out through its back, where they serve to attach wires extending to a galvanometer.



E, tube of vulcanite; C, plug of wood; *m*, *n*, copper rods; A, thermal junction; B, concave mirror; D, stop.

The thermal junction is made by uniting with hard solder a bit of watch-spring and a bit of flattened copper wire. The whole is then worked to a ribbon 1^{mm} wide, $\cdot 03^{mm}$ thick and 25^{mm} long. The two ends of this ribbon are then soldered to the two copper rods so that the junction may be midway between them.

A concave mirror of glass, silvered upon first surface, is so secured upon the plug that the junction is exactly at its focus. The front of the tube is provided with an opening of any convenient size, and stops to limit the diameter of the entering ray.



TANEY COUNTY, MISSOURI, METEORITE.

The accompanying sketch will make the details clear. Its working has been very satisfactory. It requires no longer to return to zero than for the galvanometer needle to come to rest, and is correspondingly rapid and dead beat in its action. It is much more sensitive than a thermo-pile of the same exposed area.

An instrument in actual use having an opening of 8^{mm} deflects its galvanometer 30 divisions of its scale when the hand is held a foot from the opening. A lighted match at six feet drives the needle around to its stop.

Bowdoin College, Brunswick, Me., Nov. 8, 1887.

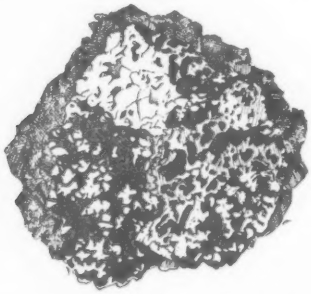
ART. LIII.—*On some American Meteorites;* by GEORGE F. KUNZ. (With Plate X.)

1. *The Taney Co., Missouri, Meteorite.*

DURING June 1887 a meteoric mass came into my possession and through the kindness of Miss Hattie Payne, of Lamar, Ark., I learned that it was taken about thirty years before from a spot in latitude 36° 35′ north and longitude 93° 12′ west of Greenwich, near Miney, Taney Co., Missouri, eleven miles S.E. of Forsyth and over sixty miles from Limestone Valley, Ark. Miss Payne also stated that about thirty years ago a meteorite passed over the boundary line between Arkansas and Missouri and was supposed to have fallen near by. After considerable search it was believed to have been located on a farm eleven miles S.E. of Forsyth, whence it was taken sixty miles to a farm in Limestone Valley, Newton Co., Arkansas, on the supposition that it was of value. As it was decided not to be of meteoric origin, however, it remained unnoticed for twenty-eight years, except that a few gun sights were made from it by some of the curious neighbors. A portion of it was sent to the writer and he at once secured the balance of the mass. For figures see Plate X. The mass measures $34^{cm} \times 35^{cm} \times 29^{cm}$ and at the smaller end is 12^{cm} high. Its weight is 197 lbs., (89·796 kilos). It is similar to the Hainholz, Westphalia, iron* is one of the Syssidères of Daubrée and of the Logronite group of Meunier. Two large crystals of olivine are present, one measuring 10×8^{cm} and another 4×6^{cm}; this part being so much lighter in color than the rest of the mass and so much more easily detached that the larger crystals has been almost entirely picked out to a depth of 5^{cm}. At one corner of the mass there is an inclosure of augite measuring 7×4^{cm}. This

* Pogg. Ann., 1857, vol. c, p. 342.

is gray and granular in structure, and has all the appearance of a common gray pebble inserted in the iron. The surface of the meteorite is deeply pitted and in many spots traces of a black crust are still visble; the pitting measures 1 to 4^{cm} across. On one side a fungoid growth has slightly stained it green. Microscopic sections were made, and in these it was seen that the olivine did not occur in separate crystals, but rather in aggregations of irregularly shaped grains, surrounded by brown ferruginous veins and with banded anorthite grains interspersed



here and there. These aggregations are full of black microlites, glass masses and needle-shaped clear crystals, and are imbedded in the metallic iron without any border of alteration. The boundary line is perfectly sharp, fresh and distinct, in which characteristic it differs from the meteorite from Powder Mill Creek. The olivine appears to be fresh, but is clouded with the brown ferruginous stains abundantly scattered through it and between the grains. The following analyses were kindly furnished me by Mr. J. Edward Whitfield, and were made before its identity with the Newton County, Arkansas, meteorite was suspected. He says:

"The analysis of the metallic portion is as follows:

Fe	89·41
Ni	10·41
Co	·29
P	·16
	100·27

Of the rocky portion I have made an analysis of the whole part, i. e., not separated as soluble and insoluble, but with the metallic part separated.

The analysis is as follows:

SiO_2	45·88
Al_2O_3	7·89
FeO	19·73
CaO	6·02
MgO	17·96
NiS	1·67
FeS	·54
	99·69

"From the nickel and sulphur and iron we have the percentage corresponding to the formula (Ni, Fe) S, for the troilite. Taking the piece as it was received the specific gravity is 4·484. Of the finely ground rocky portion, free from metallic particles as far as possible, I have made quite a number of analyses, to learn the nature of the insoluble mineral, and as far as I can judge it is enstatite only and the soluble part is a lime-iron silicate with considerable Al_2O_3. Of the insoluble in dilute hydrochloric acid the following is the analysis.

SiO_2	52·39	= ·87	
Al_2O_3	7·11		
FeO	14·68	·20	·81
CaO	4·49	·08	
MgO	21·33	·53	
	100·00		

"Ratio of $SiO_2 : R'O = ·87 : 81$ which agrees pretty well with enstatite; but here the MgO is replaced by as much FeO and the presence of Al_2O_3 makes the ratio vary a little from the normal 1 : 1. Deducting all the S as NiS and the Fe to correspond to the remaining S from the soluble part, we have for the percentages of the soluble the following:

SiO_2	26·95
Al_2O_3	17·69
FeO	35·98
CaO	15·98
MgO	3·40
	100·00

"The little MgO here probably comes from the slight solubility of the enstatite."

On looking up the literature on this subject the following facts presented themselves.

Prof. Shepard states,* in reference to the Forsyth, Taney Co., Missouri iron, that his first information regarding it was derived from N. Aubushon of Ironton, who reported that a small specimen of very curiously knitted, malleable ore, of a white color resembling silver had been sent him two or three years before by

* This Journal, II, vol. xxx, 1860, p. 205. He had so little of it, that it does not appear on any catalogue, not even in the Shepard collection at Amherst College, Mass.

a person residing near the locality. He had had an analysis of it made and found it to consist of iron and nickel. Prof. Swallow the State geologist of Missouri found it to be composed of similar constituents. He continues: "The mass evidently belongs to the rather rare group of amygdaloidal meteoric irons, in which like those of Hainholz and Steinbach, the peridotic ingredient preponderates over the nickelic iron. Its specific gravity is 4·46. The iron is remarkable for its whiteness, while the peridot is of a well marked green color and distinctly crystalline. No pyrite is visible in the very small fragments examined. It is reported that two very considerable masses of this meteorite were found buried in the soil upon a hill side, and that they are at present secreted under the belief that they contain silver."

Prof. E. J. Cox says * he was informed by Mrs. Scott of Van Buren, that when in the N.W. part of Crawford County, near Penneyoits, Sulphur Spring, attending a barbecue, on July 4th, 1859, about noon, a shower of small meteorites fell on the roof of a cabin half a mile distant, one of which was sent to Capt. Albert Pike, of Little Rock, Ark.† Although twenty-eight years have elapsed nothing has been heard of any of these pieces, in spite of frequent inquiries.

Dr. J. Lawrence Smith‡ describes a meteorite as coming from Newton Co., Ark., but without giving more exact data, as follows: "The original has not been obtained. The only fragment of it being in the hands of Judge Green was given to Prof. Cox, who has kindly presented it to me. The weight of this fragment is 22½ ounces and was evidently broken off from one corner of the mass, as it presents three of the original surfaces. The specific gravity taken on different pieces varies from 4·5 to 6·1. By mechanical means and the aid of the magnet the following minerals were separated: nickeliferous iron, chrome iron, sulphuret of iron, hornblende, olivine and carbonate of lime."

ANALYSES.

NEWTON CO., ARK., J. LAWRENCE SMITH.

Olivine.		*Hornblende.*		*Iron.*	
Silica	42·02	Silica	52·10	Iron	91·23
Alumina	·46	Alumina	1·02	Nickel	7·21
Prot. iron	12·08	Prot. iron	16·49	Cobalt	·71
Magnesia	47·25	Prot. manganese	1·25	Copper, Phosphorus	tr.
	101·81	Magnesia	29·81		
		Alkalies	·24		99·15
			100·91		

* Geol. Reconnaisance, Arkansas, 1860, p. 308.

† Learning that Capt. Albert Pike, now General Albert Pike, resided in Washington, D. C., a letter was sent him in regard to the meteorite; he replied on Nov. 15th, 1887, as follows: "Sometime before the war, and before 1860, I think, I was at Pennyoits Sulphur Springs, some 25 or more miles from Van Buren, and learned that a meteorite had shortly before fallen near there, and had been found. Afterwards a piece of it was procured for me, which became prize of war. I don't know where the piece that I had is or where the residue of the mass went to."

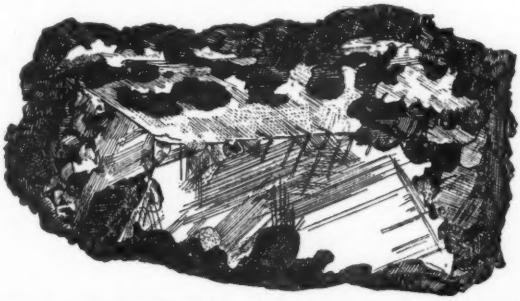
‡ This Journal, xl, pp. 213–216, 1865.

It is very evident that the carbonate of lime which Dr. Smith found in the Newton Co. meteorite was due to an abrasion on the limestone rock, with which the locality where it was last taken, Limestone Valley, is abundantly supplied. Not a trace of this substance could be detected by Mr. Whitfield. The difference in the analyses of the Taney Co. mass will suggest itself at once, but we must consider that these often vary considerable in different parts of the same meteorite?

From all the facts obtained it is quite evident that these two pieces are parts of one and the same meteorite, which originally fell near Miney, in Taney Co., Missouri. It is possible, also, that the report of the fall of a number of meteorites on the 4th of July, 1859, above referred to, may give the time at which this mass fell, although no definite statement can be made to that effect. To Dr. O. W. Huntington and Mr. J. H. Caswell the author is under obligations for comparative and microscopic data, and to Mr. Whitfield for the analysis, and to Prof. F. W. Clarke for his courtesy in regard to the same.

2. *The Chattooga County, Georgia Meteorite.*

This mass of meteoric iron was found by Mr. W. J. Fox, about the 27th of March, 1887, on his farm in Holland's Store,* Chattooga Co., Ga. In all 27 lbs. (12·5 kilos) were found but the mass fell into the hands of parties from Alabama who were interested in developing iron mines, and was broken into pieces, three of which, weighing 9, 1½ and ½ lb. respectively, came into my possession, while the balance were worked into nails, horse-shoes and other forms by the local blacksmiths. It is one of the Hexaëdrische Eisen of Brezina, with twinning laminæ, No. 60, and one of the Caillite group of Meunier. The specific gravity as obtained by me is 7·615.



The smaller of the Whitfield Co. masses was found 20 miles N.E., and the larger mass 14 miles N.E. of Dalton, while this was found 30 miles S.W. of Dalton, (see map.) The fracture

* Latitude 34° 22′ North, longtitude 85° 26′ West of Greenwich.

is in part very granular, resembling in this respect the Seelasgen iron.* But the cleavage is in some parts very marked, and the two cleavage angles measured were 120°. In breaking up the iron four cleavage planes were obtained, one of the surfaces being 2^{cm} square and two others 3^{cm} square each, which were very smooth and bright. On etching with weak nitric acid the iron turned dark and markings became visible that had all the appearance of scratches due to imperfect polishing. In fact they were at first mistaken for scratches and the iron was twice repolished. In this respect, and in its hardness, it very closely resembles the Butcher irons.† They are the Neumann figures, the result of a twinning of the cube described by Tschermak.‡ The iron was then treated with strong nitric acid and evenly dissolved away, with the exception of the (see figure) eating out of one of the layers parallel with the cleavage face and undoubtedly the same with it. It has included round masses of troilite distributed quite plentifully through it, from 3 to 8^{mm} in diameter, and on polishing down the side of the iron these were found so much altered as to be scarcely distinguished as such, rather resembling compact limonite. Lawrencite, chloride of iron, is very plentiful in this iron and collected in large drops on the surface and rolled off into the tray containing the specimen.

The following analyses were kindly furnished by Mr. J. Edward Whitfield, of the U. S. Geological Survey.

	1	2	3	
Fe	94·67	94·66	94·60	
Ni			4·97	
Co			·21	
P			·21	(·207)
			99.99	

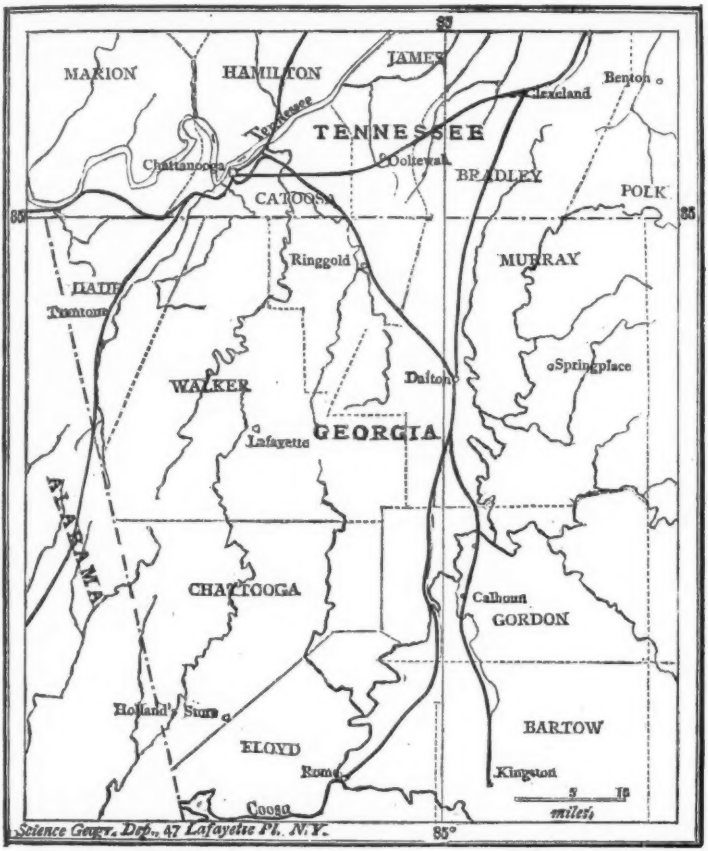
The specific gravity of the largest piece received is 7·801.

There is a very slight trace of S and C but hardly enough to determine.

This iron does not bear the slightest resemblance to either of the Whitfield County, Georgia, irons,§ and is a white iron, whereas the Walker County, Alabama, iron‖ has a bluish cast and was found over 100 miles due east.

The writer desires to extend his thanks to Mr. Whitfield for the analyses and to Prof. F. W. Clarke for his courtesy.

* Pogg. Ann., 1848, vol. lxxiii, p. 329.
† This Journal, 1869, II, xlvii, p. 383 (see figure).
‡ Akad. Wiss., Wien, lxx, i, page 449.
§ This Journal, III, xxi, p. 286, 1881; III, xxvi, p. 337, 1883.
‖ Ibid., I, xlix, p. 344.



3. *Is the East Tennessee Meteorite from Whitfield Co., Georgia?*

While looking over the literature on the Chattooga Co. meteorite the writer happened upon some facts which seem to give the locality of a very interesting mass of meteoric iron, i. e., that of East Tennessee. Of the original locality we know little definitely. Dr. F. A. Genth says* in regard to an undescribed meteoric iron from East Tennessee:

"Under the date of May 11, 1878, the late Julius E. Raht, of Cleveland, Tenn., wrote me as follows:—I send you to-day a small piece of meteoric iron which was broken from a mass weigh-

* Proc. Acad. Nat. Sci. Philad., 1886, p. 366.

ing 50 lbs., which fell about 8 years* ago near the state line of Georgia, 10 miles† from here (Cleveland). * * * *

The Museum Record of the Academy simply notes the date of its reception as Oct. 24th, 1876, that it came from the mountains of East Tennessee, and weighed 254 lbs. All my efforts to obtain fuller information about its fall and discovery have proved unsuccessful."

Prof. C. U. Shepard, in his description‡ of the meteoric iron of Whitfield County, Ga., makes the following statement:

"Some time during the autumn of 1860, Mr. H. C. Hamilton, of Dalton, Ga., noticed an unusual atmospheric phenomenon in that region. A bright light shot across the heavens, followed by a loud report, which created great alarm among the people, many of whom supposed the end of the world had arrived. * * * A *large mass of iron* supposed to be a meteorite was found about ½ mile from this one in the year 1862 (the 117 lb. Whitfield mass). It was sent to Cleveland, Tenn., where it appears to have been lost sight of."

The following are analyses of the two irons, No. 1 is that given by Dr. Genth, and No. 2 that of Prof. C. U. Shepard, Jr.:

	1. East Tennessee. Sp. grav. 7·521.	2. Whitfield Co., Ga. Sp. grav. 7·986.
Iron	89·93	94·66
Copper	0·06	
Nickel	8·06	4·80
Cobalt	0·56	0·34
Phosphorus	0·66	
Sulphur	not det.	P, Mn, Cr, traces
	99·27	99·80

These analyses do not agree as closely as might be expected, but in some other respects the points of resemblance are close.

A large etched section of the East Tennessee iron in the possession of the writer shows several cracks completely filled with rust, indicating, as Prof. Genth says he had observed, that there are large quantities of chloride of iron in the mass. The large mass of Whitfield Co., deposited in the National Museum, rusts, cracks and exfoliates exactly like the above. The Widmanstätten figures are also identical. A new analysis of the Whitfield Co. mass would perhaps serve to establish their identity.

Cleveland, Tenn., is 28 miles N.E. of Dalton, Ga., and the Whitfield Co. iron was found 14 miles N.E. of Dalton, the latter place being very near the state line (see map). Mr. Raht

* This corresponds with the time given by Mr. Hamilton, of Dalton.
† Given by Prof. Shepard.
‡ This Journal, xxvi, 336, 1883.

also writes that a 50 lb. (?) mass fell* 10 miles from Cleveland near the state line, which locates it very near where the Whitfield Co. mass was found. Cleveland, Tenn., the place from which the East Tennessee mass is said to have been sent, is on or near a railroad line. The mass was probably sent there for sale or to be worked at one of the iron furnaces.

When we consider that the war was then in progress and that even for some years after the war the intercourse which had been broken off was not resumed, it is not unreasonable to suppose that this mass may have lain unnoticed for several years.

4. *On a mass of Meteoric iron from Waldron Ridge, Claiborne Co., Tenn.*

During March, 1887, Judge Fulkerson of Tazewell, Claiborne Co., Tenn.,† received, from some prospector in the vicinity, specimens of what was supposed to be an ore of iron. Some of these were sent to Dr. J. M. Harbison and Prof. W. E. Moses, of Knoxville, Tenn., to Dr. J. S. Newberry, of the School of Mines, Prof. Safford, of the University of Tenn., and others. Through the kindness of the three former gentlemen specimens have come into my possession. This iron is one of the Caillite group of Meunier. In structure it is one of the octahedral irons. On the largest piece, weighing 15 lbs., this is very marked, as it is scarcely altered. All the other pieces, weighing collectively several pounds, have been detached from around this piece, which was apparently the center of the mass. The smaller pieces all show considerable weathering. Several perfect octahedrons and one tetrahedron were obtained by simply breaking the iron off with the fingers, it separating very readily at the cleavage plates between which, in nearly all instances, were thin folia of schreibersite. Troilite was also observed as well as graphite, clearly suggesting that this iron is identical with the Cosby Creek,‡ Cocke Co.,§ the Sevier Co.,‖ the Greenbrier Co.¶ mass in the British Museum, and the Jennies Creek, Wayne Co., W. Va.,** meteorites, which, although independently described, are evidently parts of one meteorite, as suggested by Huntington,†† which either exploded on entering our atmosphere so that the fragments traveled according to their impetus, or else threw off these

* The masses having been supposed to be of meteoric origin it may have been rumored that they had recently *fallen* when they had simply been *found*, and 250 pounds may have been meant instead of 50.

† Latitude 36° 46′ North; longitude 83° 35′ W. of Greenwich.

‡ Troost, this Journal, I, xxxviii, 253.

§ Shepard, ib., I, xliii, 354.

‖ Ib., VI, iv, 83.

¶ Huntington, in British Museum Collection.

** Kunz, ib., III, xxxi, 145.

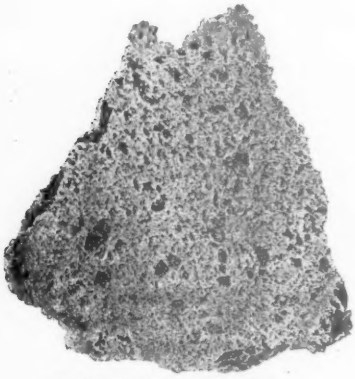
†† Ib., III, xxxiii.

pieces at various periods in its course. In all there was perhaps about 3 pounds, although it was supposed at first that there was a whole mine of it. The other pieces were obtained after the 15 lb. piece, and not one of them weighed more than a pound.

5. *On the Powder Mill Creek Meteorite.**

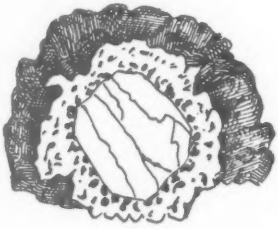
Through the kindness of Mr. Moritz Fischer, of the Kentucky Geological Survey, I am now the possessor of a piece, weighing over 2,000 grams, of the meteorite which Colonel Sublet and Mr. Lenoir found on the farm of Elihu Humber, at Powder Mill Creek, about eight miles west of Rockwood Furnace, on the eastern slope of Crab Orchard Mt., latitude 35° 50′ north, longitude 84° 45′ west of Greenwich, in Cumberland Co., Tenn. (Rockwood being in Roane Co.). It resembles very closely the Hainholz, Westphalia, 1856, and the Newton Co., Arkansas, irons, now the Taney Co., Mo. (see p. 470). It is scarcely distinguishable from the latter (see fig. 1), except that in the latter the grains are larger and more readily defined. The specific gravity was found to be 4·745.

1.



Polished section, natural size.

2.



Magnified 20 diameters.

Chloride of iron (lawrencite) is present in considerable quantities, and on a number of sections which had been cut and polished it was perceptible within a short time. It collected in small beads on the piece itself, which will undoubtedly lead to

* This meteorite is identical with the one described as the *Rockwood meteorite* in the last number of this Journal, and only facts additional to those already published in Mr. Whitfield's article are here given. A part of this meteorite was exhibited and described by the author at a regular meeting of the New York Academy of Sciences, May 30, 1887. The name Powder Mill Creek was given to it because it fell in Cumberland Co.; Roane Co., in which Rockwood is situated, being adjacent to this.

a rapid disintegration unless the iron is coated with varnish or some other preservative. Even small fragments have already become seamed, suggesting that the fall is recent. "Microscopic sections were prepared, and in the ground-mass of metallic iron were seen clear crystals of anorthite and olivine. The former are transparent, with inclusions of glass having fixed gas bubbles, and of many needle-shaped microlites, and some of larger size. The former microlites are probably enstatite, while some black quadratic sections may be chromite or magnetite. The twinning bands of the anorthite are sharp and distinct. The olivine crystals have greenish, brownish veins of alteration (perhaps induced by the lawrencite) with inclusions of glass, microlites and an abundance of black grains of picotite (see figure 2). These grains are occasionally arranged symmetrically around the crystals as a border, outside of which is usually a grayish, partly opaque mass between the crystal and the metallic iron. This grayish mass is an alteration of the olivine which in many cases has taken place in the entire crystal and in others leaving only a small center of clear olivine."

To Mr. J. H. Caswell the writer is indebted for the above microscopical data and to Mr. Moritz Fischer for information and assistance in procuring the pieces of the meteorite.

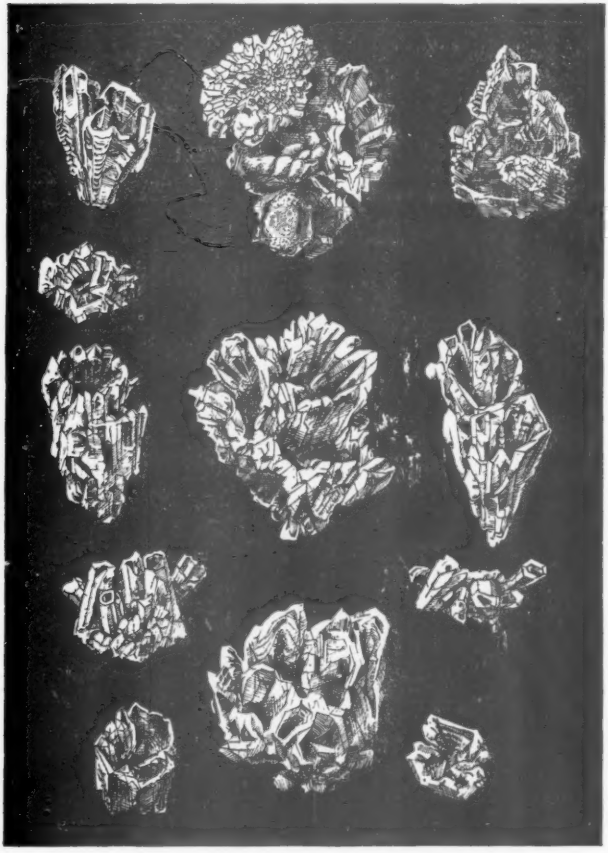
ART. LIV.—*Mineralogical Notes*; by GEORGE F. KUNZ.

1. *Rhodochrosite from Colorado.*

RHODOCHROSITE in rich red, transparent crystals has been found in the John Reed mine at Alicante, Lake Co., Colorado. One specimen shows forty crystals lining a cavity 8^{cm} in width; these crystals are simple rhombohedrons (R), 12^{mm} across, a number of them being entirely transparent. Some cleavage pieces are as pellucid as red Iceland spar and show the same strong double refraction. One of them measured 10^{mm} by 20^{mm} and weighed 2 grams. This is the first locality that has yielded crystals of such magnitude and transparency. The specific gravity is 3·69 and the hardness 3·5; it is not scratched by calcite but is by Bilin aragonite. An analysis, by Mr. James B. Mackintosh, yielded the following results:

MnO	58·325
FeO	3·615
CaO	none
MgO	trace
CO^2 by difference	38·06
	100·00

It also occurs opaque, massive and cleavable, enclosing bright cubic crystals of pyrite at times 25mm across. Some of the crystals are opaque while others are of a transparent flesh color. At the Ulé mine, Lake City, Col., crystals, both opaque and flesh-colored, rounded and curved like dolomite, are found on an argentiferous galena. Some crystals were observed 23mm across. The faces are not polished but rough like the *himbeerspath*. With the spectroscope there is considerable absorption of the citron, forming a decided dark band between the yellow and the green. With the dichroscope the ordinary image is a salmon-pink and the extraordinary light yellow sherry color.



Crystals of Hollow Quartz

2. *Crystals of Hollow Quartz from Arizona.*

These crystals are found about 3 miles southwest of the town of Pinal, in Pinal Co., Arizona. They occur in a sandstone which has been traced for about one mile; it is penetrated in many places by spherules of obsidian $\frac{1}{8}$ to 3 inches across, which cover the ground like a shower of large hailstones. These quartz crystals are of exceptional interest from the fact that they are mere walls surrounding hollow spaces much larger than the area of the wall itself (see figures on adjoining cut). In some instances the crystals radiate from a central point; numbers of the detached crystals show this very strikingly. At the point of attachment there often appear small nuclei of chalcedony, and in some instances, the crystals radiate from this chalcedonic center so as to form true quartz flowers, as shown in the second figure. Quite often they are single individuals hollowed out, leaving only the smallest edges of what ought to be a pyramid, and the interior of the crystal being hollow to the base in a few instances. They are not cavernous like the Poretta, Italy, and Burke Co. quartz. As hollow quartz their characteristics are sufficiently marked to isolate them from those found at any other locality.

It is very evident that they are the result of a crystallization from a solution, and the chalcedony being rounded and coated white, has every appearance of stalagmitic growth, fading imperceptibly into quartz. In some few instances the quartz crystals have a coating of chalcedony at their terminations. The sides of the prismatic faces are striated and have all the unevenness of those made up of crystalline plates. The hollow crystals themselves are usually made up of six flat individuals so symmetrically arranged as to leave the center hollow, the whole often being one distinct crystal. Many of the crystals are terminated with the pyramid face R only; -1, when present, is usually very small and only slightly developed. Some, however, have both R and -1. In general appearance they resemble the amethyst of Schemnitz, Hungary. The figures were all carefully drawn and are of the natural size.

In this connection it may be mentioned that from near Crouch's Mill, Gaultney's township, Alexander Co., North Carolina, the writer obtained a beautiful group of quartz radiating from one of these chalcedonic nuclei and forming a complete flower that measured 7^{cm} across. None of these crystals, however, were hollow.

3. *Hydrophane from Colorado.*

A white opaque variety of hydrophane, in rounded lumps, from 5^{mm} to 25^{mm} in diameter, with a white, chalky or glazed coating somewhat resembling the cacholong from Washington

Co., Georgia, has recently been brought from some Colorado locality. For its power of absorbing liquid it is quite remarkable. When water is allowed to slowly drop on it, it first becomes very white and chalky, and then gradually, perfectly transparent. This property is developed so strikingly that the finder has proposed the name "Magic Stone" for it, and has suggested its use in rings, lockets, charms, etc., to conceal photographs, hair or other objects which the wearer wishes to reveal only when his caprice dictates. The specific gravity of several specimens was taken with the following results:—Nos. 1–3 were slabs 2^{mm} thick, No. 4 was a natural lump with glazed coating.

	Dry. Grams.	Wet. Grams.	Water abs.	Weight(in water).	Spec. grav.
1.	·880	1·342	·588	·463	2·110
2.	·644	·934	·416	·3385	2·091
3.	·730	1·109	·379	·382	2·097
4.	1·8745		1·0595	·864	2·191

The weight was taken both dry and wet, and it will readily be seen that this hydrophane absorbs more than an equal volume of water.

A remarkable nugget of Silver.

Gen. A. G. Greenwood recently called my attention to a nugget of native silver weighing 606½ ozs. Troy, one of sixty that have been found at the Greenwood group of mines in the state of Michoacan, Mexico. The other nuggets weighed from one to thirty-five pounds each. The large nugget is entirely worn except in cavities where some of the crystals are rounded and the form is still visible. It is almost pure silver, scarcely a trace of any gangue rock being discernible. This specimen was found on the surface, and in its original state is said to have weighed 12 lbs. more. It is one of the most remarkable nuggets of silver ever found. The geological formation is a limestone with outcroppings of limonite.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the Study of Chemical reactions by means of the Electrometer.*—Bouty has sought to make use of the electric resistance of solutions in order to determine the chemical changes produced when two dissolved salts are mixed together. Thus, for example, when potassium sulphate and sulphuric acid are mixed in solution, the observed resistance is greater than that calculated on the supposition that no action takes place between them. Hence it follows that the normal salt combines with the acid to

form hydrogen-potassium sulphate. Moreover, as the solution becomes more dilute the quantity of this acid salt diminishes until ultimately it is completely converted into the normal salt. The molecular resistance of the hydrogen-potassium sulphate, which varies according to the same law as that of other salts, is proportional to a factor $1+Km^{\frac{1}{2}}$; the proportion of this acid salt in the solution being given by the expression $y=Ym^{\frac{1}{2}}$. The somewhat high value of $m^{\frac{1}{2}}$ in the formula for the molecular resistance, indicates the dissociation of the acid salt. If the molecule of hydrogen-potassium sulphate constitute a single electrolytic molecule, its resistance at the limit will be that of the normal salt, and in solutions containing not more than one-tenth of a gram-equivalent per liter the difference between the resistances of the two salts will be a vanishing quantity compared with the much greater conductivity of the free acid. It is found that the proportion of the acid sulphate, calculated from the observed resistances of the liquid, increases with the temperature. Hence, combining this with the previous result, it follows that hydrogen-potassium sulphate is more stable in warm and concentrated solutions. At the same temperature and with the same degree of concentration, the proportion of the acid salt increases with an increase in the relative proportions of either the free acid or the normal salt, an excess of the normal salt being more favorable to this change than an excess of sulphuric acid. These results agree with the results of Berthelot's thermo-chemical investigations. Possibly the great variations in the resistance of abnormal salts and of free acids may be due to a progressive dissociation of complex electrolytic molecules, according to the same law as that of hydrogen-potassium sulphate; but this effect, while added to, will not modify the law of the variation of the resistance of salts which are normal in all degrees of dilution.—*C. R.*, civ, 1789, 1839; *J. Chem. Soc.*, lii, 882, October, 1887. G. F. B.

2. *On the Detection of minute traces of Carbon dioxide.*—In order to detect minute traces of carbon dioxide RÖSSLER has modified the ordinary method as follows: The lower end of a test tube is drawn out to a narrow tube which is then turned upward and to one side, the opening being about a centimeter above the bend. A piece of thin glass tube, rather larger in diameter than the test tube, is drawn out to a gradual taper so that while at top it rests upon the test tube, its lower and capillary end is from 1·5 to 2^{cm} from the bottom of the this tube. The substance to be examined is placed in the bend of the test tube. The capillary funnel is then partly filled with baryta water so as to leave a drop hanging from the lower end, and is placed in the test tube. Upon dipping the apparatus in hydrogen chloride, the acid enters the capillary opening of the test tube and comes in contact with the substance in the bend. The evolved carbon dioxide comes in contact with the baryta water and produces the well known turbidity. To test the delicacy of the apparatus, sodium carbonate was mixed with sodium chloride and introduced into it. When the former was 0·005 gram the turbidity was strong. With 0·0005,

it was very distinct; and with 0·00005 gram, it could be seen on close observation. This corresponds to 0·02 milligram carbon dioxide. By placing starch and potassium iodide in the capillary funnel, sulphurous oxide is easily detected in this way. Concentrated ferrous sulphate may thus be used to detect nitric acid vapors, a mixture of nitrate and common salt being used with sulphuric acid. Hydrogen sulphide is thus detectible by lead acetate, ammonia by copper sulphate, nitrous acid by potassium iodide, etc.—*Ber. Berl. Chem. Ges.*, xx, 2629, October, 1887. G. F. B.

3. *On the Heat of Combination of Tellurium.*—BERTHELOT and FABRE have determined the heat of combination of several different varieties of tellurium by pulverizing them finely, dissolving them either in bromine or in water saturated with bromine, and measuring the thermal disturbance thereby produced. Crystallized tellurium, prepared by volatilization in hydrogen evolved 66·66 calories. Tellurium precipitated by sulphurous acid 42·584 calories; precipitated from alkali tellurides by the action of the air or other oxidizing agents, 66·78; precipitated from hydrogen telluride by oxidizing agents, 67·01. The last two varieties are identical with the first, therefore; and hence, taking the mean of the three at 66·776 calories, it follows that the conversion of crystallized tellurium into the amorphous variety develops 24·192 calories. Consequently tellurium, like sulphur and selenium, exists in two perfectly distinct states. Molten tellurium, thrown into cold water and then treated as above, gives numbers varying from 44 to 56 calories, indicating that it is a mixture of the crystalline and amorphous varieties. It is rather a curious fact that while the change from amorphous to crystalline tellurium absorbs heat, the corresponding change for selenium evolves heat, and the change in the case of sulphur gives rise to no sensible thermal disturbance at the ordinary temperature; though at a high temperature it is positive and is probably negative at a low one.—*C. R.*, civ, 1405–1408; *J. Chem. Soc.*, lii, 761, September, 1887. G. F. B.

4. *On the occurrence of Aluminum in the Ashes of Flowering Plants.*—It has long been the current opinion of chemists that aluminum is not a normal constituent of flowering plants, the occurrence of this element in the ash of phanerogams being generally attributed to adhering particles of the soil. YOSHIDA has undertaken a set of careful analyses of several seeds and grains with a view to decide the matter, and he finds in the hull or skin of a pea (*Soja hispida*) 0·268 per cent of alumina, in the red bean (*Phaseolus radiatus*) 0·096, in rice (*Poddy*) 0·189, in wheat 0·106, in barley 0·140, in millet (*Panicum italicum*) 0·272, and in buckwheat, 0·113 per cent. The cotyledons of the pea alone of all the substances analyzed contained no alumina.—*J. Chem. Soc.*, li, 748, October, 1887. G. F. B.

5. *On Ammonio-cobaltic permanganates.*—KLOBB has described a series of new salts, which are exceedingly beautiful and at the same time highly explosive, and which are obtained by

adding cold solutions of potassium permanganates to ammoniacal solutions of metallic salts. With silver nitrate the compound $(NH_3)_2AgMnO_4$ is obtained as a dark violet crystalline powder which detonates on warming. Similar salts are given with copper, cadmium, nickel and zinc. The most interesting, however, are the salts of the ammonio-cobalt bases, particularly luteocobalt. Its permanganate is obtained by mixing concentrated solutions of luteocobalt chloride $(NH_3)_{12}Co_2Cl_6$ and potassium permanganate at a temperature not above 60°, in the proportion of one molecule of the former to twelve of the latter. The new salt separates on cooling in little black quadratic octahedrons having a brilliant luster. If powdered and carefully warmed in a test tube these crystals decompose suddenly with incandescence, but if heated without pulverization, they incandesce with detonation, the tube being shattered into fragments. When struck with a hammer, the crystals detonate violently; even powdering them in a mortar being sometimes accompanied by dangerous crepitations. This salt has the formula $(NH_3)_{12}Co_2(MnO_4)_6$. Other compounds have been obtained in which hydrogen chloride and hydrogen bromide partially replace the hydrogen permanganate. A beautiful salt of this kind has the composition $(NH_3)_{12}Co_2(MnO_4)_4Cl_2 . (KCl)_2$. It forms dark violet hexagonal plates bearing often low six-sided pyramids, and sometimes grouped together in the form of six-rayed stars resembling snow flakes in shape. These salts are less explosive than the others.—*Ann. Chim. Phys.*, VI, xii, 5–33, September, 1887. G. F. G.

6. *On the Character of Solution.*—MENDELÉJEFF has given a definition of solution and has illustrated his views in the particular case of solutions of ethyl alcohol in water, the data being here very accurate. "Solutions" he says "may be regarded as strictly definite atomic chemical combinations, at temperatures higher than their dissociation temperature." Just as definite chemical substances may be formed or decomposed at temperatures higher than those at which dissociation commences, so in solutions; they can be either formed or decomposed at ordinary temperatures. Moreover, the equilibrium between the quantity of the definite compound and of its products of dissociation is defined by the laws of chemical equilibrium, which laws require a relation between equal volumes and their dependence on the mass of the active component parts. If, therefore, the above hypothesis of solution be correct, comparisons must be made of equal volumes, and the specific gravities are the weights of equal volumes. Moreover, we must expect the specific gravities of solutions to depend on the extent to which the active substances are produced. Hence the expression for specific gravity s, as a function of the percentage composition p, must be a parabola of the second order: $s=N(p\pm a)(100-p\mp a)$
This may be represented by the general equation

$$s=C+Ap+Bp^2$$

Between two definite compounds which exist in solutions, it is to be expected that the differential coefficient $\frac{ds}{dp}$ will be a rectilinear function of p: i. e., $\frac{ds}{dp} = A + 2Bp$. This conclusion can be verified by experiment. Moreover, it gives the means of ascertaining what the definite compounds are which exist in the solution. Applying now the method to the solutions of H_2O and C_2H_6O, three definite compounds are found to exist: C_2H_6O with $(H_2O)_{12}$ and $(H_2O)_3$, and (H_2O) with $(C_2H_6O)_3$; containing 17·56, 46·00 and 88·46 per cent of alcohol respectively. The rectilinear character of the differential coefficient is shown by plotting the results in a diagram. Calculating the constants of the parabolas, expressions are obtained for the specific gravity in terms of the percentage composition, the values calculated from which agree very closely with those obtained by experiment. The author finds tho above hypothesis respecting the rectilinear character of the differential coefficient to be correct, not only for solutions of a hundred different salts but also for solutions of H_2SO_4, of NH_3, of HCl, and other similar substances. Indeed up to the present time he has not met with a single solution which is an exception. At present he is investigating minutely, solutions at low temperatures; and in connection with Teeshenko he has already obtained the compounds $C_2H_6O(H_2O)_{12}$ and $C_2H_6O(H_2O)_2$ in the solid form and crystallized; the former at $-17°$ C., and the latter in a mixture of solid CO_2 and ether.—*J. Chem. Soc.*, li, 778–782, October, 1887. G. F. B.

7. *On the Chemical action of Bacterium Aceti.*—Brown has continued his experiments on the chemical action of the ferment *Bacterium aceti*. He had shown that mannitol is completely decomposed by it, levulose being the main product. He now finds that its isomeride dulcitol suffers no change whatever by the action of the ferment. Glycol is readily oxidized to glycollic acid. Glycerol is converted into carbon dioxide and water. Erythrol undergoes no change whatever. Hence, by transforming dextrose into mannitol by sodium amalgam, and mannitol into levulose by the ferment, dextrose may be converted into levulose. Moreover, Brown finds that the cellulose formed by the ferment *Bacterium xylinum* from levulose, yields a dextrorotatory sugar on treatment with sulphuric acid.—*J. Chem. Soc.*, li, 638, July, 1887. G. F. B.

8. *On the light emitted by glowing solid bodies.*—According to Dr. J. Draper, all solid bodies begin to glow at the same temperature, which is about 525° C. As soon as a platinum wire, heated by an electrical current, reaches a temperature slightly above 525°, the light emitted shows a spectrum from the line B to *b*. When the temperature reaches 645°, the spectrum extends from B nearly to F. At the temperature 718°, it extends from B to G. And at the temperature 1165°, the spectrum has nearly the full extent of the solar spectrum. Dr. Draper was led

to believe that the increase in the spectrum was toward the more refrangible end. H. F. Weber, from observations on incandescent electric lights, has discovered that bodies begin to glow at a much lower temperature than the limit set by Draper.

The first trace of light was seen at about 390°, and was of a spectral gray color. As the temperature increased, the spectrum of the glowing carbon filament did not increase more rapidly toward the violet than toward the red; but increased equally in both directions. The first rays which are perceived by the eye come from the part of the spectrum which sends forth the maximum energy when the filament is heated to its highest point. The experiments were repeated with metallic filaments raised in temperature by non-electrical methods, and the results obtained by electrical currents were thus confirmed. Weber maintains also that all bodies do not begin to glow at the same temperature, as the results of Dr. Draper seemed to show. A plate of platinum began to send forth gray light at 391°; while a gold plate showed the same character of light at 417°. In another experiment, a platinum plate began to glow at 396°, and an iron plate at 378°. The paper of Weber is followed in the *Annalen der Physik* by one on the same subject by Fr. Stenger, who confirms in general the results of Weber; but suggests that the varying sensitiveness of the observer's eye to various colors and to their degrees of intensity may play an important rôle in the determination of the relative rate of increase of the spectrum of glowing bodies.—*Ann. der Physik und Chemie*, No. 10, 1887, pp. 256–275. J. T.

II. Geology and Mineralogy.

1. *On the Location of some Vertebrate Fossil Beds in Honduras, C. A.;* by Frank L. Nason.—The locality referred to is situated about twenty-seven miles east of the Ulna river and in the Nigrito valley. This valley is bounded on the east and west by ranges of lofty mountains varying in height from three to six thousand feet. The mountains gradually draw together north and south, enclosing a basin from forty to fifty miles in length and about twelve in width. From the northern extremity of the valley two streams (one of them the Rio Roman), join to form the Rio Comayagua, which empties, later, into the Ulna, thus forming the outlet or drainage for the valley.

At the foot of the mountains are ranges of foot-hills not over one thousand feet in height. These foot-hills are evidently the débris washed from the mountains and into the lake which at that time filled the valley. A rather striking feature of these foot-hills, as well as of the valley, is the almost entire absence of tropical vegetation. While the mountains are clothed with the customary tangled growth, these hills and the valley have only straggling thickets of guava and other shrubs, though quite a dense growth of grass is abundant which furnishes pasturage

for large herds of cattle. The nature of the soil is the only sufficient explanation of this change of vegetation.

The soil is a very tough clay filled with weathered bowlders of all sizes. An examination of the bowlders showed them to consist largely of a coarse-grained diorite from the decomposition of which the clay is formed. Aside from the numerous pyramid-shaped piles of stones scattered around, examinations were made in the beds of gullies with perpendicular walls, cut by the water during the rainy season.

It was while examining the stones in one of these gullies, that my attention was attracted by several huge bones projecting from the walls. My interest was at once awakened on account of this being a possible intermediate link between the great Tertiary Lake formations in the United States and Mexico, and those in the U. S. of Columbia. I had only my geologist's hammers with me, and with these no headway could be made in extricating these bones from the tough clay. Had I succeeded in extracting them no means were at hand for carrying them to the coast twenty miles away.

Upon inquiry of our host, through our interpreter, he informed me that the entire skeleton of a huge animal had been washed from the soil a few years before, but that all but a few fragments had been destroyed by the inhabitants who were more curious than scientific. A few fragments were shown me, and as there were bits of much decomposed ivory among them, I concluded that the remains were those of Mastodons or other allied species.

I was further informed that a very perfect skeleton had been taken by the government to the college at Santa Barbara: Business prevented my visiting this town and I am unable to confirm the report. There is great likelihood that this locality is very rich in vertebrate remains, and that search for them would be comparatively easy on account of the natural sections cut by the streams which are dry except during the rainy season.

Nor would the paleontologist alone find working material. The foot-hills, to which I have alluded, bear numerous traces of former inhabitants, as well as demonstrate that the valley was formerly a lake. On the summit of these hills are very frequently found great heaps of bivalve shells about the form and size of the ordinary fresh-water clam. They could not have been taken from the modern streams as they are all rapid, and I noticed only such univalves as are commonly found in such places. Among these shell heaps are frequently found flint and stone implements, and also bones and the rude stone images often found among the Aztec ruins. These last two statements I am unable to vouch for as my limited time allowed only a hurried search.

It may be interesting to note that, to me, the Nigrito appears to be but one of a chain of lakes which extended from the Ulna river to near the river Poyas, about two hundred miles east.

There is no doubt but that a scientific party, with leisure, would be richly repaid for time spent in exploring this section of

country, less known, perhaps, than any other country in Central America. Abundance of vertebrate fossils, and relics of past human races as well as plain though fast vanishing marks of the early explorers of this country, fill these valleys from the Ulna river to Truxillo.—*Communicated to this Journal by the author.*

2. *Professor L. F. Ward's Synopsis of the Flora of the Laramie group.*—The following notes on Professor Ward's valuable work are from a letter of August 9, 1887, to the author from Mr. L. Lesquereux, received from the former by the editors.

I am delighted with your excellent exposition, and thank you heartily for the pleasure and instruction I have received from it. Of course I do not agree with you in all your views, nor even with all your assertions; though that proves nothing against you. I will only mark a few of the points on which I do not fully agree with you.

Considering the climatic circumstances, as indicated by the local presence of some species of plants, you say, p. 438, that the Palms of the lower districts of the Laramie indicate a difference of climate greater than can be accounted for by the small difference of latitude. From Dr. Newberry's "Later Extinct Floras" and from Dr. F. V. Hayden's remarks, it appears that the largest specimens of fossil Palms (*Sabal Campbellii*), have been found in the Fort Union Group; near the mouth of the Yellowstone, I suppose. From the Raton Mountains, where plants of the same kind are found in abundance, there is a difference of 22° of latitude; and in that distance there is no marked difference in the characters of the vegetation of the Laramie group, not even in the absence of Ficus or of Cinnamomum at certain localities, and therefore only a mere local difference. *Cinnamomum affine* Lx., a close relative of *C. polymorphum*, is not rare in the higher strata of the Laramie, where you place the locality of Carbon. Your exposition of the flora of the Iron Bluff, so remarkably varied and different in each of the successive strata, is the best possible indication of the extraordinary variety of the groups of floras of the Laramie, though separated they may be at the same localities by their intermediate strata. The same difference is remarked at Golden where, apparently at nearly the same horizon and at little local distance, one of the strata bearing vegetable remains has, in abundance, leaves of Platanus, *Ficus Goldiana*, and *Ficus spectabilis*, while the other has mostly leaves of some species of Populus and none at all of the species of the first examined bed. This, it seems, shows the impossibility of considering the difference in the vegetation of the Laramie at different localities as influenced by mere climatic circumstances.

In the arrangement of your table of distribution which has been prepared with the greatest care and has certainly demanded long and careful research, I have to remark only on the admission of the Credneria beds of the Upper Quader into the Senonian. They are placed by Hosius at the very base of the Senonian, from which he has a few leaves of Credneria. But the type of these leaves is older; it pertains to the lower Quader or the Cenoman-

ian, and for many geologists most of what is considered Upper Quader is of the same age as the Lower. In any case, if even your splendid leaves, which you refer with doubt to the genus, were true Credneria, their relation should be indicated as with the Cenomanian or with the original type, not with the derived one in the Senonian. This, however, is of little importance, as really the characters of these leaves are, in my view, far different from those of Credneria, especially on account of the basilar position of the lowest secondaries (or lateral primaries) and the absence of the secondaries, or rather tertiaries, underneath. It is also from the basilar point of attachment of the lateral primaries that I can not recognize the identity of the two leaves of pl. xl, fig. 8 and 9, with *Platanus Heerii*, whose leaves are moreover entire.

There is, however, a species of the Dakota group which shows the closest relation (even identity?) to species of the Laramie. It is *Populus elliptica* Newby., which can scarcely be separated by distinct characters from *P. cuneata* of the same author. This last species is therefore evidently of Cenomanian type, the first being the ancestor of *Populus arctica*, *P. Richardsoni*, *P. Zaddachi*, *P. speciosa*, *P. amblyrhyncha*, *P. Daphnoides*, *P. Oxyrhyncha*, etc. The type is not seen with the Cretaceous of Greenland; not even in the Patoot Senonian. But it is reproduced, and frequently so, in the Tertiary of Greenland, as you well know. This with a number of other affinities tend to show, in my opinion at least, that if there has been a migration of the plants between America and Greenland at the Cenomanian period, the migratory movement has been northward, not southward. But this question is out of place here.

I received last Saturday the No. 37 Bulletin of the Survey with the figures and descriptions of your Types of the Laramie. This small volume is very handy and for study of paleobotany more valuable still than the synopsis. Though I have sent an acknowledgment of receipt to the Survey, I take the liberty to thank you heartily for the production of a book which will be of great assistance to paleobotanists of the present and especially for the future American students of vegetable paleontology. What an immense amount of material is still in reserve for those who follow in your footsteps and collect specimens along the Yellowstone! How is it that you did not find any Palms near Fort Union, or along the Yellowstone?

Very sincerely and thankfully yours, LEO LESQUEREUX.

Professor Ward has sent the editors the following remarks on Mr. Lesquereux's letter.

To certain of the above criticisms, the force and legitimacy of which I freely concede, the following reply may be made.

I was not so fortunate myself as to find Palms in the Fort Union beds, and the only ones coming from them that are known to me are those determined by Dr. Newberry to which Professor Lesquereux refers, and which represent only a single species, *Sabal Campbellii* Newby., which occurs so abundantly at Golden,

Colorado. This species will be found properly entered in the table of distribution on page 467, and the total absence of Palms in the Fort Union group was not maintained. They certainly must have been rare in this section, whereas they are found in nearly all the beds in Colorado and Wyoming. The same disproportion is found in the genus Cinnamomum.

As regards the age of the Credneria beds of Europe I can only say that those of Brankenburg, where Zenker first found the genus, and of Quedlinburg, where a number of species occur, are placed by Schimper (Traité de Paléontologie végétale, vol. iii, p. 673) in the Senonian of D'Orbigny, as are also those of Westphalia, since so much more thoroughly studied by Hosius and Van der Marck, who located them in the lower portion of that member. Most of the other Credneria beds lie in the Lower Quader and should doubtless be correlated with the Cenomanian. Such are those of Silesia, Saxony, Bohemia and Moravia. Schimper, however, would place Moletein, where Heer's *C. macrophylla* was found, in the Turonian (op. cit., vol. iii, p. 59). I do not certainly maintain with any force that my *C. daturæfolia* represents a true Credneria, and I have set forth in my Types of the Laramie Flora (p. 98), which Professor Lesquereux had indeed received but probably had not yet examined on this point, all the objections raised by him to such a reference, together with some others. I merely wished to make a suggestion sufficiently startling to attract attention to this remarkable form and to call out the opinions of others as to its proper affinity.

LESTER F. WARD.

3. *A pot-hole of remarkable size in Archbald, Pa.*—The pot-hole recently brought to light in the coal formation at Archbald, Lackawanna County, Pa., according to an account of it in the Scranton Republican of November 4, 1887, has a depth of forty-five feet, and a somewhat ovoidal section with the largest diameter twenty feet. Its lower end was discovered in 1883 in the process of coal mining, and since then it has been cleared and used as an air-shaft. It is on the property of Mr. E. B. Hackley, who has taken precautions, at considerable expense, to ensure its preservation. It is supposed to be a product of the latter part of the Glacial era, and to have been made by a great water-fall descending through a crevasse in the glacier. This view implies that the glacier of the region did not move on as much as ten feet during the centuries in which the excavation to a depth of forty-five feet was going forward; or else that the local conditions occasioning the crevasse were such as to keep the crevasse stationary, and without eroded sides from the water-fall, during those centuries, notwithstanding the movement of the glacier. Either supposition seems to be almost incredible. The other explanation supposes a great river, made from the melting of the glacier, to have done the excavation by the usual river methods, that is, while flowing in torrents along a shelving, rocky shore.

4. *The Lake Age in Ohio, or some Episodes in the Retreat of the N. American Glacier.*—A paper on this subject by Professor

E. W. Claypole, illustrated by maps, is contained in the Transactions of the Geological Society of Edinburgh, 1887, p. 421.

5. *Carboniferous Fossil Corals.*—Mr. James Thomson has a paper on the genus *Lithostrotion*, its limits, characters, and species. Ibid., p. 371.

6. *Primordial Fossils from Mt. Stephens, N. W. Territory of Canada;* by Dr. C. Rominger.—The species embrace Trilobites of the genera *Ogygia*, *Embolimus*, *Menocephalus?*, *Conocephalites*, *Bathyurus?*, and *Agnostus*. The beds contain also species of *Obolella*, *Orthis*, *Kutorgina*, *Theca* or *Hyolites*.—*Proc. Acad. Nat. Sci. Philad.*, 1887, p. 12.

7. *The Teachings of Geography:* suggestions regarding principles and methods for the use of Teachers; by ARCHIBALD GEIKIE, Director General of the Geological Survey of the United Kingdom. 202 pp. 12mo. London and New York, 1887, (Macmillan & Co.)—Dr. Geikie's geological science has enabled him to give special value to this work for instructors in geography.

8. *A North Carolina Diamond.*—Mr. G. F. KUNZ states that a diamond weighing 4½ carats or 873 milligrams was found on the Alfred Bright farm in Dysartville, McDowell County, N. C., in the summer of 1886. It is quite perfect, but not pure white, having a faint grayish-green tint. In form it is a distorted hex-octahedron with partial twinning (see figure of two views). Its specific gravity is 3·549, and it measures ten millimeters in length and seven millimeters in width. A number of small stones exhibited as diamonds have been found at Brackettstown, near by, but they have proved to be either transparent zircon or smoky-colored quartz. A visit to the locality served to authenticate the facts of its discovery. None of the minerals ordinarily associated with the diamond were found at the locality, and the opinion is expressed that the diamond must therefore have been transported from distant higher ground in the vicinity during a heavy freshet.

9. *Herderite.*—The rare mineral herderite, long known only in a few unique specimens from Saxony (1828) and later (1884) found at Stoneham, Me., has been recently obtained from Mursinsk in the Ural. The crystals are associated with yellow orthoclase, smoky quartz, tourmaline, muscovite and topaz; in habit they resemble the original mineral more nearly than that of Stoneham. —*Berwerth. Ann. Mus. Wien*, 1887.

10. *Periclasite.*—The rare oxide of magnesia (MgO), occurring only at Monte Somma, has been identified at the manganese mines of Nordmark in Sweden. Blowpipe trials show the presence of a little manganese and iron.—*Œfv. Ak. Stockholm*, Sept. 14, 1887.

III. BOTANY.

1. *Monographiæ Phanerogamarum Prodromi*, Vol. v. *Pars secunda: Ampelideæ*, auctore J. E. PLANCHON, has at length appeared. It occupies 350 pages; and it represents a great

amount of labor, the permanent value and complete acceptance of which cannot be adjudicated off-hand. The plan of merging all the forms into one genus, *Vitis*, has been abundantly tried, not with very satisfactory results,—partly, it may be, because the groups have not been well worked out. Professor Planchon, a most experienced and keen botanist, who has especially investigated the Vines for a good many years, has very naturally tried the other tack, and has developed the Linnæan *Vitis* and *Cissus* into ten genera. The principles upon which he has proceeded, as explained in the preface, are wholly legitimate; and one could wish that they have been successfully applied. This, only use can determine. We may be confident, however, that if this monograph had been in the hands of the authors of the latest Genera Plantarum, they would not have bodily adopted its conclusions, although they would have been much helped by the elaborate investigations, and might have seen their way to admit three or four genera. They would not have trusted over-much to the difference between polygamo-diœcious, polygamo-monœcious and partly pseudo-hermaphrodite, hermaphrodite and probably some pseudo-hermaphrodite, and hermaphrodite or rather physiologically polygamo-monœcious and with some blossoms pseudo-hermaphrodite,—differences which must be shadowy,—nor to variations in the mere shape of style and stigma. And as to the disk, which should be more tangible and hopeful, we gather from Planchon's synopsis and from our own observations that there are only three types. In the true Grape-vines, the disk is represented by nearly distinct and free nectariferous glands, alternate with the stamens. In most other Ampelideæ, it is cupular or annular (entire or crenate or lobed,) with base or lower half or more adnate to the base of the ovary, but at least the margin or lobes free. In the Virginia Creeper there is really no disk at all, as was first noted by Dr. Torrey in his Flora of the Northern States, in 1824, and insisted on in the Flora of North America, in 1838, and again in the Genera Illustrata, where there are correct figures. Dr. Planchon expresses the same opinion in essence but in different language, i. e., "Discus obsoletus ovarii basi plane adnatus et tantum colore proprio subdistinctus." We could not make much of the color; but the tissue does thicken more or less, and possibly may become obscurely nectariferous; but the flowers are not attractive to bees, as the allied species from Japan is. In the latter while there is equally no *hypogynous* disk, there is much thickening of nectariferous tissue over all the lower part of the ovary more or less in longitudinal ridges, the whole 'plane adnatus' throughout. Now we should make more of these three types than Dr. Planchon does. For the first goes with the Calyptrately Caducous corolla and polygamo-diœcious flowers of true *Vitis*. The third with disk, if so called, wholly confluent with the ovary itself, belongs to and includes all of the few known species (including Planchon's *Landukia*) which have the striking biological character of climbing by the dilatation and adhesion of the tendril tips; and their flowers are 5-merous, essentially hemaphrodite, and

with expanding corolla. The second type of disk goes with 4-merous and some 5-merous flowers with corolla expanding in anthesis, that is, to the genus *Cissus*. We do not see the way to break this up into genera—certainly not on the number of parts, for this varies in some species, and while *C. stans* is 5-merous, the closely related *C. orientalis* is 4-merous. However it may be with some exotic groups, we must restore our two species, which formed part of Michaux's *Ampelopsis*, to the genus *Cissus*. Under that view the generic nomenclature is clear. The genus *Ampelopsis* (Michaux, p.p. and Torr. and Gray) is to be maintained on the lines long ago laid down in this country, and now reinforced, for those species which are popularly well-known under this name. We do not feel obliged to defer to any work of Rafinesque as late as the year 1830. But, as to the present point, it seems to us that when Dr. Planchon followed him in the appropriation of one part of Michaux's *Ampelopsis*, he should also have adopted Rafinesque's name for the other part, viz: *Quinaria*, instead of making a new name, *Parthenocissus*, the former name being free for use. Under our view both names are superfluous. As to true *Vitis*, it remains to be seen whether it will be at all possible to distinguish twenty or more North American species. Perhaps Engelmann allowed quite as many as can be defined. But Planchon's long and conscientious labors upon the genus and the family must be most helpful even where his conclusions are not at once accepted. A. G.

2. *Report on Botanical Work in Minnesota for the year* 1886 occupies Bulletin No. 3 of the Geological and Natural History Survey of that state. There are, first, papers by Professor L. H. Bailey, sketches of the flora of Vermillion Lake and other northern parts of Minnesota, with lists of plants collected. Monotony and paucity of species characterize the flora between Lake Superior and the international boundary; and this is taken "as an illustration of the law that species decrease with the increase of latitude," as if there were no other sufficient explanations of the floral poverty of that tract. Mr. Upham adds a supplement to his list of the flora of Minnesota, adding about two dozen species. The first is the *Anemone nudicaulis* Gray, in Bot. Gazette, reprinting the article, but with no further information. We had hoped that the station would ere this have been revisited. A. G.

3. *The Course of Practical Instruction in Botany*, by Professor Bower and Professor Vines, of which the first part was published in 1885, is now completed on the same lines by the issue of a second part for the Bryophyta and Thallophyta. This part is the work of Dr. Bower, and, of course, it is an excellent manual for the botanical laboratory. It is published by Macmillan & Co., London and New York, pp. 144, 18mo. A. G.

4. *Grasses of North America for Farmers and Students*, etc., W. J. Beal, Professor of Botany and Forestry in Michigan Agricultural College. An octavo volume replete with information, not only about grasses but clovers and such-like forage plants, and many other matters, scientific and practical. It

begins with "protoplasm," it ends with "debris," or bits of wisdom—poetic and prosaic, secular and biblical—left over when the book was done, "for want of a suitable place to use them." There is, last of all, a bibliography, in which *the* Robert Brown is not discriminated from *a* Robert Brown, author of a Manual of Botany, owing, no doubt, to the exceeding brevity of citation; while, by some odd mishap, Bentham and Hooker's Genera Plantarum is said to be published at Berlin. But it is hardly gracious to notice such blemishes, nor to complain of proof-reading and form, since the book was printed at Lansing, yet not badly in most respects. The numerous figures are mainly particularly good, not only those of grasses drawn by Mr. Scribner, but also the microscopic illustrations by Sudworth. The short and good chapter on the Fungi of Forage Plants is contributed by Professor Trelease; and the original figures, if not fine, are fairly effective. Not content with giving, in this volume, a practical account of the principal forage grasses, with their physiology, morphology, cultivation and management,—in which "it is hoped that the farmer or general reader who has never studied botany will find much to interest and help him"—and the hope is amply warranted—the author announces a second volume, to contain the description of all known grasses of North America, 700 or more species. This is a more formidable undertaking, whether the treatment is to be mainly popular and practical, or more scientific, or both combined. A. G.

5. *Serjania Sapindacearum Genus monographice descriptum;* von L. RADLKOFER, was published in 1875 in the Transactions of the Royal Bavarian Academy at Munich, and it gained the Candollean quinquennial prize, as well it might. This accomplished systematic botanist has now published, in a later volume of the same series a detailed supplement, of nearly 200 pages and 9 plates, the last a distribution map. Moreover, the *Fish-inebriating Plants* have been taken up by Dr. Radlkofer (Sitzungsberichte der Bayer. Acad. Wiss., xvi, 1886, p. 380) *apropos* in the first instance to two notable species of *Serjania*, treated of in his monograph of that genus. To the present paper is added an *Index plantarum ad pisces capiendos adhibitarum*, arranged under their natural orders. The list amounts to 154 species, besides twenty others which, being known only under popular names, are dubious. A. G.

6. *The British Moss Flora*, its indefatigable author, Dr. Braithwaite, has, in Part x, completed the first volume of. It runs to 315 pages, with 44 plates (such as only Sullivant could rival), all executed by his own hand. This admirable work is published by the author, at 303 Clapham Road, London. We are informed that the sale so far covers the actual outlay, and that it will be continued. Would that we could have such a moss-book, with figures of all our North American species; but as that may not be as yet, and as the species are largely the same, our bryologists should thank Dr. Braithwaite for all he is doing for them. A. G.

7. *Pittonia, a Series of Botanical Papers*, by EDWARD L.

GREENE, Assistant Professor of Botany in the University of California, part 2, just come to hand, bears the date of July, 1887, and carries on the volume to p. 93. The opening pages explain for the benefit of the uninstructed, the meaning of the convenient catch name *Pittonia*, seemingly with some idea of retrieving an injustice done by "the rising Swedish authority [who] arbitrarily set aside the then old and well established name Pittonia and put his own new and more cumbrous Tournefortia in its place." Well, in his day we know that Linnæus felt very much at liberty in such matters, and wished the genus to bear the real cognomen of the botanist it commemorated. Plumier himself dedicated his genus to Josephus Pitton Tournefort, which is also the name on the title page of the *Institutiones*. On the title pages of the two earlier works it is simply Mr. Pitton Tournefort, and there is no intimations that he ever used the *de*. So we have rather to bless Linnæus for transgressing a rule which did not then exist. In a following article Professor Greene gives an account of a monstrosity of *Collinsia bicolor* having mainly regular corollas, and thereupon suppresses Nuttall's genus *Tonella* at once. Next two new genera are made out of portions of *Krynitzkia*, and *Piptocalyx* is set up anew; a good number of miscellaneous new species are described, and a detailed account of *Carpenteria Californica* is given, quite unaware that this shrub is now rather commonly cultivated in Europe, and has been figured in the Botanical Magazine, The Garden, Gardener's Chronicle, etc. That the parts of the flower, at least the calyx, are sometimes 6 or 7 is stated in the original description, indicated on the plate, and referred to in the Botany of California, in the second volume of which a habitat is given. An interesting account of a botanical excursion to the Island of San Miguel concludes the number. A. G.

8. *Catalogue provisoire des Plantes Phanerogames et Cryptogames de la Basse Louisiane, Etats-Unis d'Amerique*, par A. B. LANGLOIS, Pointe-a-la-hache, Louisiana.—This is a list of the plants at the mouth of the Mississippi, collected and determined by the Rev. M. Langlois, a parish priest whose zeal, knowledge, and success in botany are notable. It is well to have a catalogue of the plants of that district. Among the species which have been borne to that shore from the south are *Cuphea glutinosa*, *Polygonum Meisnerianum*, *Cyperus Surinamensis*, and *Luziola Peruviana*. A. G.

9. *The Development of the Ostrich Fern, Onoclea Struthiopteris*, by Dr. DOUGLASS H. CAMPBELL, is a neat memoir published by the Boston Society of Natural History (iv, no. 2, 1887) pp. 35, and 4 plates, 4to, as the Walker Prize Essay of 1886. It appears to be a faithful piece of detailed work. It would have been well to append a statement of what had already been done in nearly the same field. A. G.

10. *Studi botanici sugli Agrumi e sulle piante affini.* Rome, 1887. By O. PENZIG.—This elaborate memoir of nearly 600 pages of text and an atlas of 58 folio plates appeared in the Annali di Agricultura, published under the direction of the Italian

Minister of Agriculture, Industry and Commerce. The author states that he has been at work on the question proposed by the minister, "the study of the structure, vital functions and diseases of the *Citri*," since the year 1879, and the memoir is the most complete work on the subject that has as yet appeared. A third of it is devoted to the morphology and histology of *Citrus* and related genera; this is followed by chapters on the comparative anatomy of the *Aurantiaceæ*. Part third treats briefly of the chemical properties of the *Citri;* and part fourth is devoted to diseases of cultivated species caused by fungi and insects. The latter subject is discussed minutely and forms the most important part of the memoir. In two previous papers the author described 166 species of fungi parasitic on *Citri*, and the number has been increased to 190 in the present volume. The plates of fungi appeared originally in the *Fungi Italice autographice delineati* of Saccardo. The memoir ends with a full bibliography and index; the whole is beautifully printed and does great credit to the Italian government. W. G. F.

11. *Elements of Botany*, by Asa Gray. New York, 1887. 8vo, pp. 226.—This title was given by Dr. Gray to his first educational publication, more than fifty years ago. The earliest treatise has grown into the Text-Book, while this new work is designed to replace the well-known Lessons in Botany. No confusion is likely to result from the revival of the older name after the lapse of half a century, and its assignment to a wholly different book. The new work differs in many features from any of its predecessors. It embodies the grammar of Organography, the first principles of Vegetable Physiology, and of Botanical classification, and gives with the help of an adequate glossary, directions for the description of plants. The bringing so much within so narrow a compass has been accomplished by giving the kernel of every topic without any husks. The "Elements" is designed to be the companion and interpreter of the "Manual" and kindred works, and will be bound up with these as the grammar and dictionary and handy work of reference. It is equally adapted to the wants of classes and of those who are obliged to study by themselves. G. L. G.

12. *Elements of Botany;* by Edson S. Bastin, A.M. Chicago, 1887. 8vo, pp. 282.—The author of this treatise states that there exists "a genuine need for some work on botany better adapted to the wants of our high schools, academies, and colleges of pharmacy and medicine than any in present use." The numerous excellent introductory works on botany, now before the English-reading public, have not made this want severely felt; but so long as teachers differ in their preferences as to methods of presenting a subject to their pupils, so long will some place be found for every new claimant upon the attention. Professor Bastin's work is methodical and trustworthy, and its scope is sufficiently wide for those whom it has been prepared to serve, except that it is not particularly pharmacological, as would be expected from the Professor in the College of Pharmacy at Chicago. G. L. G.

IV. Miscellaneous Scientific Intelligence.

1. *National Academy of Sciences.*—The following is a list of the papers presented at the meeting held at Columbia College, New York, Nov. 8–10, 1887.

T. C. Mendenhall: Seismoscopes and Seismological investigations.

E. D. Cope: On the primary specializations of the True Fishes; On the mechanical origin of the structures of the hard parts of the Mammalia.

Wm. A. Rogers: A study of the behavior of metals under variations of temperature.

T. Sterry Hunt: Chemism in its relations to temperature and pressure; Progressive series in Chemistry.

J. D. Dana: Kilauea, a Basalt Volcano.

Henry Mitchell: Circulation of the Sea through New York Harbor.

Ogden N. Rood: On a study of Color Contrast.

W. K. Brooks: On the relative variability of men and women; On a new form of reproduction in Meduse; On the Lucayan Indians.

A. M. Mayer: Experiments in measurements of statical electricity in absolute units; On Potential as measured by work, a mathematical discussion.

Theo. Gill: A comparison of Antipodal Faunas.

W. P. Trowbridge: On a discovery recently made in connection with the flight of birds.

E. C. Pickering: On the determination of star magnitudes by photography.

A. Hall: On the constant of aberration.

J. S. Newberry: The Cretaceous Coals of Western North America; The future of Gold and Silver Production.

S. P. Langley: The Temperature of the Moon.

A. A. Michelson and E. W. Morley: On a Method of making the wave-length of Sodium light the absolute standard of length.

OBITUARY.

Gustav Robert Kirchhoff, Professor of Physics at Berlin, died on the 17th of October, in his 64th year. His death removes from the ranks of physicists one who has long held a foremost place and to whom the world of Science owes some of the most important steps of progress it has made. His first paper was published in 1845, and from that time until the later years of his life, when his health was seriously impaired, his contributions were numerous, varied in subject, and of a high order. First of all in importance come his contributions to spectrum analysis, a department of Physics largely created by him and by Prof. Bunsen, his colleague at Heidelberg where the work was done; this was in 1859 and 1860. Prior to this date his attention had been largely devoted to electrical subjects, and his papers upon Ohm's law, the discharge of the Leyden Jar and kindred topics, are all of great value. Still another series was devoted to investigations on the equilibrium and motion of elastic solids, and his work entitled Lectures on Dynamics (1876), is well known. A collection of his papers was issued at Leipzig in 1882.

Oscar Harger, assistant in the Paleontological department of Yale University, died on November 6th. A notice is deferred until a later number.

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